

Three Essays on the Economics of Fertility

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in the University of Michigan
2016

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DEDICATION

In memory of my mother, Bernie.
For her love, laughter, and endless support –
I am eternally grateful.

ACKNOWLEDGMENTS

That this is even possible is a testament to the many people who have greatly helped me along the long and winding road to get here. I am especially grateful to my committee—Martha Bailey, Charlie Brown, David Lam, and Pamela Smock—for their guidance, time, incredible patience, and unwavering support. Thanks also to the Michigan Economics Department for providing me the space and flexibility to finish this dissertation. Thanks to Pamela Smock, for the opportunity to work on her IFSS project and learn so much about fertility survey data, and to Dan Leeds who valiantly stepped in during my absence and kept me sane throughout the project.

None of this would be possible without the advice, support, kindness, and encouragement of my advisor, Martha Bailey. Working as her research assistant taught me more about applied research, problem solving, and good coding practices than any other single experience in or outside of graduate school. Her insightful and knowledgeable comments helped shape all of the work in this dissertation. I will be forever grateful for her mentorship and guidance.

Chapter III: I am exceptionally grateful to my coauthor, Brad Hershbein, not only for his elegant code and wit, but also for moving this research forward when I was unable to. We thank the participants at the University of Michigan Labor Seminar and the annual meetings of the Population Association of America for helpful comments and suggestions.

Chapter IV: The research in this paper, joint with Martha Bailey, was supported by the Herman Family Foundation, the Robert Wood Johnson Health Policy Scholars Program, and the University of Michigan Institute of Social Research's GSRA tuition support program. Liz Cascio generously sent us the 1970 and 1980 county-group crosswalks, enabling our analyses. We also thank John Bound, Charlie Brown, John DiNardo, Daniel Eisenberg, and David Lam for helpful comments and suggestions. Danielle Gwinn at the Consulting for Statistics, Computing, and Analyzing Research (CSCAR) provided essential GIS assistance. Exceptional research assistance was provided by Allie Davido.

Thanks to my wonderful colleagues at USAA who provided helpful comments and constant encouragement, and to the USAA Educational Assistance program for funding my return to graduate studies.

My continued existence is thanks to the wonders of modern medicine and the excellent care of my many teams of oncologists, particularly Drs. Deb Schrag and Karen Fasciano at Dana Farber and Dr. Brian Bednarski at MD Anderson.

Thanks to my father, Lincoln, who taught me to love data and experiments, and showed me the importance of persistence. His genuine celebration of non-zero progress made my many years of barely detectable forward movement vastly less discouraging. Perfection remains, as always, elusive.

Finally and most importantly, I am eternally grateful to my husband, Greg, who never allowed me to quit—his encouragement and non-stop cheerleading kept me going when I so desperately wanted to give up. Only through his truly herculean efforts taking care of all other aspects of our lives did I have enough time to finish this dissertation. He is my Powdermilk Biscuit—giving me the strength to go out and do what needs to be done. I could never have done this without his patient love and support.

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CHAPTER I

Introduction

The twentieth century experienced unprecedented changes in childbearing behavior. Economists, sociologists, and demographers have all developed models and explanations of these dramatic shifts in fertility, approaching the question from different angles. Economists have tended to emphasize the demand side, focusing on changes in wages, in particular women's wages, which increase the opportunity cost of raising children, or on changes in income, which could lead couples to substitute higher child quality in place of quantity. Demographers, on the other hand, tend to emphasize the supply side, focusing on the revolutions in contraception, sexual mores, and women's rights of the 1960s and 1970s.

In my research, I am interested in better understanding the determinants of fertility. My dissertation presents three chapters focusing on different aspects of American fertility. Chapter II focuses on the demand side, using a natural experiment to investigate the effects of an exogenous income and child price shock on the demand for children among women in Alaska. The Permanent Fund Dividend (PFD) is a program that distributes a share of Alaskan state oil revenues to every resident of Alaska, including newborn children, beginning in 1982. Using Vital Statistics and decennial census data, I employ a difference-in-differences methodology to compare the period fertility rates of Alaskans with those of other Americans before and after the introduction of the PFD. Overall, I find that the fertility effects of the PFD are statistically significant but relatively modest and short-lived, consistent with the Easterlin relative-income

hypothesis, suggesting that subsequent cohorts of Alaskan women adjusted their material aspirations to include PFD payments. I calculate a short-run price elasticity of demand for children of -1.6, in line with other estimates in the literature.

Chapters III and IV examine the supply side effects of changes in access to the birth control pill—among college women in the 2000s and among married women in the 1960s, respectively. Chapter III, joint with Brad Hershbein, exploits the unexpected and significant increase in the price of birth control pills at college health centers following minor changes in wording in the Deficit Reduction Act of 2005. Using two different data sets, the National College Health Assessment and the National Survey of Family Growth, we employ multiple empirical strategies for identification and find consistent results across data sets and methodologies. We find that the policy change reduced use of the Pill by 4 to 6 percent among all college women, and that the decline was two to three times as large for college women who lacked health insurance or carried large credit card balances. We supplement our data with a unique survey on how and where college women fill birth control prescriptions and find that Pill usage is largely price inelastic, with a price elasticity bounded between -0.09 and -0.035.

Chapter IV, joint with Martha Bailey, investigates the causal impacts of access to the Pill on the period and completed fertility of women in the late 1950s and early 1960s. We exploit differences in state-level anti-obscenity statutes first described in Bailey (2010) that rendered the sale of the Pill illegal in 13 states. We employ a difference-in-differences strategy to examine changes in fertility outcomes between women living within and beyond 50 miles of a permissive state border. We show that this distance measure captures most of the variation in Bailey (2010), and find little within-state impacts of distance on period fertility. We find

suggestive evidence that easier access to the Pill may have led women to alter the timing of their births, but find no significant longer-term effects on completed fertility. Together, these papers examine how two important factors, income and contraceptive access, have contributed to women's childbearing decisions over the past 60 years.

It is important to note that a better understanding of the determinants of fertility is not a merely academic concern. Policy-makers have long been interested in levers to manipulate fertility, as fertility is the driving force behind population change in the world today. Governments throughout the world interested in altering their population trajectory have implemented dozens of policies designed to influence fertility. Some developing countries have actively tried to reduce their fertility rates, in an attempt to reduce overpopulation and hopefully spur economic growth, with varying degrees of success. Out of concern for explosive population growth, China and Vietnam went so far as to impose laws that restrict the number of children a couple can have to one in China and two in Vietnam. Other countries, such as Brazil, Indonesia, and Bangladesh, have adopted less coercive approaches and worked instead to expand access to family planning.

On the other hand, many European and developed countries are facing below replacement-level fertility and have implemented pronatalist policies to increase fertility. The first child subsidy program that paid families to have children was enacted in Quebec, Canada in 1988. Other countries have since followed their lead including Israel, Singapore, Italy, and Poland. Singapore and Taiwan offer subsidized artificial reproduction technologies to help couples unable to conceive, covering up to three cycles of in vitro fertilization as well as a wide range of other infertility treatments. In addition to more traditional national subsidies,

Ulyanovsk, Russia has instituted a "Day of Conception" on September 12th when workers get the day off work in order to procreate and those who subsequently have a child on June 12th receive rewards from the government. Russia and Poland even imposed an income tax on the childless in an attempt to encourage childbearing, though Poland's was phased out in the 1970s and Russia's in 1990.

Even here in the United States, there is much debate over how our policies, such as Temporary Assistance for Needy Families (TANF), extended unemployment benefits, or mandatory contraception coverage under the Affordable Care Act will affect American fertility rates. These policies and others demonstrate the importance of understanding how income and contraceptive access can influence fertility decisions. The results of my dissertation research can be helpful to future policy-makers in assessing the likely impact of proposed interventions that alter income or contraceptive access on fertility.

CHAPTER II

Exogenous Income Changes and Childbearing Behavior: Evidence from the Alaska Permanent Fund Dividend

2.1 Introduction

The relationship between income and childbearing has long been an open question for economists. Both cross-sectional and time-series data generally show a negative relationship between income and fertility. Within countries, families with higher incomes have fewer children than those with lower incomes. In the United States, for every cohort of women born between 1928 and 1958, the relationship between fertility and husband's income is negative (Jones and Tertilt 2008). Across countries, women in high-income countries have fewer children than women in lower-income countries. Looking at developing countries over time has generally shown that childbearing declines as per capita incomes increase with industrialization. Because there are few (if any) substitutes for children, however, standard economic intuition would lead one to believe that children should be normal goods and that fertility should increase with income.

Beginning with Becker's seminal 1960 paper on the economics of fertility, economists have modeled the demand for children by maximizing a parental utility function that depends on child quantity and a composite consumption good subject to a family budget constraint. In these models, a permanent increase in non-wage income is predicted to increase the total demand for children; because prices remain constant there are no substitution effects, while

the income effect is expected to increase consumption of all goods, including children. More often, however, income changes are the result of changes in wages, which would induce a substitution effect in addition to and in the opposite direction of the income effect and would therefore result in an ambiguous impact on the number of children.

To reconcile the difference between empirical findings of a negative relationship between income and fertility and theoretical predictions of a positive relationship, Becker (1960) and Becker and Lewis (1973), introduced a tradeoff between the quantity and quality of children. In this framework, parental utility is dependent on the interaction of child quality and quantity, and thus the shadow prices of both quality and quantity are dependent on the value of the other.¹ While it has been shown that such a tradeoff could lead to the observed negative relationship between income and fertility, recent empirical work has found no evidence of a quality-quantity tradeoff using exogenous variation in twin births and sex composition (Angrist, Lavy, and Schlosser 2010).

Another strand of literature, also stemming from work by Becker (1965), adds the time cost of children into the household utility function. Because the opportunity cost of raising children increases in wages, particularly in female wages, this could explain the negative correlation between income and fertility. In these models, a permanent increase in male wages is predicted to increase the demand for children, because the income effect dominates the substitution effect. Alternatively, a permanent increase in female wages yields ambiguous predictions, as the income and substitution effects are of likely similar magnitude (Schaller

¹ An increase in quality is more expensive if there are more children because the increase must be applied to more individuals; an increase in quantity is more expensive if children are of higher quality, because higher quality children incur higher costs.

2012). Many empirical studies use changes in male employment or wages to estimate the causal impact of income on fertility. While evidence regarding the impact of male wages on fertility is far from unanimous,² more recent studies, such as Black et al. (2013) have found a positive relationship between male wages and fertility. These studies, however, assume that male wage changes do not alter the allocation of household responsibilities between partners in ways that could affect fertility decisions. Even if the production of children does not require any of the father's time, the negative relationship can still be generated through assortative matching—higher-earning men tend to attract higher-earning women, whose leisure and child-rearing costs are higher than those of lower-earning women.³

An ideal experiment to identify the causal effect of income on childbearing would be to randomly assign households different levels of permanent income, but this is impossible in the real world. The crucial challenge, identified by Hotz, Klerman, and Willis (1997), is "to find plausibly exogenous variation in proxies for the price and income concepts appearing in the theories." This paper uses exogenous variation in income and the price of children resulting from the introduction of the Permanent Fund Dividend (PFD) program in Alaska, to examine the impact of non-wage income and price changes on fertility. The PFD program annually distributes a share of the interest from state oil revenues to every resident of Alaska, starting in 1982. These dividends account for around 6 percent of total household income in Alaska,

² Shultz (1985) finds an ambiguous relationship between Swedish male wages and fertility. Lindo (2012) finds negative effects of husbands' job displacement, but they become insignificant with the inclusion of individual fixed effects.

³ Additionally, there could be other ways job loss, in particular, affects fertility through stress or degradation of marital quality.

ranging from a low of \$331 per person in 1984 to a high of \$2,069 in 2008 (in nominal dollars).⁴

In some census areas, the PFD accounts for over 10 percent of personal income (Goldsmith 2001). The payments also reduce the price of a marginal child, as each additional child will generate an additional 18 year stream of dividend payments to the household.

The introduction of the PFD is a useful natural experiment because the payments are given to all Alaska residents, and so are uncorrelated with underlying tastes for children or work, and represent a permanent change in household income. Because it does not change the opportunity cost of leisure, unlike income changes in other studies, the total effect of the PFD is thus the sum of the income effect resulting from increased household income from dividends to all existing children and the price effect resulting from the decrease in the net-of-dividend cost of additional children.

Many studies have examined the impact of changes to the price of children such as preferential tax treatment and child subsidies on fertility, and have generally found small but meaningful short-run timing effects, and limited effects on the total number of children. Studies of Quebec's pro-natalist child subsidy programs find significant short-run effects on birth timing, but few if any longer-term effects on completed family size (Parent and Wang 2007, Milligan 2005). In their study of child subsidy programs in Israel, Cohen, Dehejia, and Romanov (2013) similarly find an effect on timing, though they also find suggestive evidence that some of the effect could be due to impacts on completed family size. Literature on parental leave policies also suggests that increases in parental leave, which decrease the cost of children,

⁴ The variability is due to changes in Alaska's state oil revenues and the performance of the Permanent Fund's investments.

increase childbearing in the short run, though the impacts on completed family size vary. Lalive and Zweimuller (2009) find few if any effects of expanded parental leave on completed fertility in Austria, but Malkova (2014) shows substantial additional births from a similar program in Russia.

This paper finds that the introduction of the PFD resulted in a substantial but short-lived increase in births from 1982 to 1995, with the largest effects from 1982 to 1989. My results suggest that Alaskan women anticipated the dividend payments and increased their childbearing in advance of the checks,⁵ but quickly adjusted to the "new normal" of PFD payments and returned to previous fertility levels in a little over ten years, consistent with the Easterlin relative-income hypothesis. I find that introduction of the PFD increased the Alaskan total fertility rate by 0.59 births (25 percent) on average between 1982 and 1995, representing a short-run price elasticity of children of -1.6. The bulk of the effect is driven by second and higher-order births and by young adult mothers (aged 20 to 29).

This paper proceeds as follows. Section 2.2 discusses the history of the PFD program and the details of its implementation in 1982. Section 2.3 discusses the data sets I employ in my empirical analysis, the Vital Statistics Natality Detail Files and the Integrated Public Use Microsamples of the decennial censuses, as well as the empirical strategy used for analysis. In Section 2.4, I discuss the results and their implications for policy-makers before concluding in Section 2.5.

⁵ This is possible because the dividends were announced in 1980 and then held up in court until 1982. During that time, the only item in dispute was whether the value of payments would be proportional to length of Alaska residency; there was never a question of whether payments would be made.

2.2 Policy Background

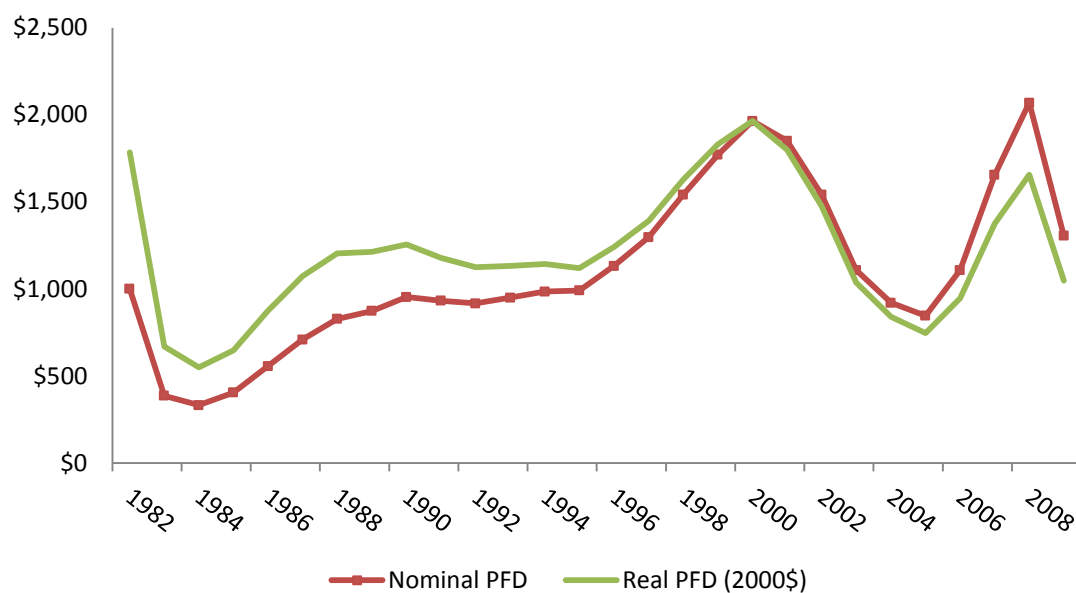
The Alaska Permanent Fund was created in 1976 by an amendment to the Alaskan State Constitution that diverted 25 percent of all royalties from the sale of state-owned natural resources into a trust fund for Alaskan citizens. In 1980, the Fund announced a plan to distribute the interest income of the fund as dividends based on the number of years of Alaska residence, one unit per year (equal to \$50 in 1980), up to a maximum of 25 units (\$1,250). Shortly after the dividend announcement, recent Alaskan arrivals Ron and Penny Zobel sued the government of Alaska, claiming that the program violated the equal protection rights of newer state citizens. The program was initially declared constitutional by the Alaska Supreme Court, and plans were made to distribute the dividends in December 1980. The Zobels, however, appealed to the United States Supreme Court, and Chief Justice Rehnquist issued a stay on November 17, 1980, preventing any dividend disbursements until the Court could hear and decide on the case. As a result of these legal battles, no dividends were distributed in 1980 or 1981. The U.S. Supreme Court eventually declared the program unconstitutional in June 1982.

During this lengthy court process, the Alaskan legislature put together a backstop dividend bill that would distribute a special first-year dividend of \$1,000 to every individual who had lived in Alaska for at least a year, and equal dividends to all eligible Alaskan residents thereafter, in the event that the original program was overturned (Rose 2008). The backstop bill became law only a few days after the Supreme Court overturned the original PFD program.

The first-year dividends of \$1,000 per person were disbursed starting on June 18, 1982 and continued throughout the latter part of 1982 (Turner 1982). Starting in 1983, dividend amounts were based on a formula equaling roughly 10.5 percent of the average Permanent

Fund profit over the previous five years. The Permanent Fund profit is dependent on both state oil revenues, which contribute to the Permanent Fund principal, and the performance of its investments. Note that no principal is disbursed as dividends, enabling the payments to continue in perpetuity in theory. From 1984 forward, dividend checks have been distributed in the last quarter of the year. Figure 2.1 shows the evolution of PFD payments over time.

Figure 2.1: Nominal and Real PFD Payments Over Time



Notes: All PFD payments are per resident, including infants. Real PFD payments are adjusted for inflation to 2000 dollars.

In the first year, dividends were available to everyone who had been a resident of Alaska for at least six months, and to all children born prior to October 15, 1982 to eligible residents of Alaska. In all subsequent years, everyone who had been a resident of Alaska for the entirety of the preceding calendar year and who declared the intent to remain in Alaska indefinitely was

eligible for the dividend.⁶ Minors who were born to or adopted by an individual who is eligible for a dividend within the two years immediately preceding the current dividend year are also eligible (Alaska Statute 43.23.005.c).

The Alaska Permanent Fund Dividend provides a unique case to examine the impact of income on childbearing for several important reasons. First, payments are exogenous to underlying unobserved preferences for children and/or work.⁷ Second, the payments are large. The 1982 dividends directly increased Alaskans' total after-tax income by \$362 million, or 6.2 percent (Knopp et al. 1984). In 1982, a family of four would have received \$4,000 (\$7,138 in 2000 dollars), which would have increased the income of the average Alaskan household by over 10 percent, or around 5 percent of average home values in Alaska at the time.⁸ Third, the payments represent a permanent change to expected household income. Though the exact value of the payments changes from year to year depending on investment performance and oil revenues, the payments are expected to continue in perpetuity, as they are made only from the profit of the Permanent Fund, not the principal.

Hsieh (2003) examines consumption behavior of Alaskan households in response to PFD payments and finds very similar quarterly consumption patterns to households in the other 49 states, even though all PFD payments in his sample were disbursed in the fourth quarter.⁹ He

⁶ Individuals who have been incarcerated or convicted of a felony at any time during the preceding calendar year, were absent from Alaska for more than 180 days for an unallowable reason, or were not physically present in Alaska for at least 72 hours during the preceding calendar year are not eligible for the dividend.

⁷ This would be violated if individuals who had a preference for larger families were incentivized to move to Alaska following the introduction of the PFD, which I address later.

⁸ Average Alaskan household income in 1980 was \$27,232; average income for Alaskan households containing a female head or spouse of childbearing age was \$28,314. Average home value in Alaska in 1980 was \$78,192; for households of childbearing age, the average home value was \$83,350. (Source: 1980 IPUMS)

⁹ Hsieh only uses data from 1984 forward, when the disbursement dates were standardized in the fourth quarter.

shows that debt balances decrease and savings balances increase more in Q4 relative to Q3 in Alaska than in other states, but finds no statistically significant impact on durable or nondurable consumption patterns. Because the government announces estimated payments 6 to 9 months in advance, Alaskan households are able to accurately predict their PFD income and smooth their consumption across the year, consistent with the permanent income hypothesis. Interestingly, Hsieh (2003) finds that the consumption patterns of those same households are highly sensitive to changes in income tax refunds, suggesting that PFD income, unlike other forms of government payments, is viewed as a permanent income change.

In addition to increasing household income, the PFD effectively decreased the cost of a marginal child, as an additional child born in 1982 would generate not only an additional \$1,000 in 1982, but also an additional PFD payment for the household for each of the next 18 years, receiving a total of over \$21,000 by 2000 (in 2000 dollars).¹⁰ While the total value of the PFD payments over 18 years far exceeds that of child subsidy programs enacted in Quebec and Israel (Milligan 2005; Cohen et al. 2013), there are notable differences between the programs. In child-subsidy programs, payments are typically made in a lump sum following the birth, whereas PFD payments are distributed over a longer time horizon (18 years). Additionally, PFD payments are dependent on the Fund's performance rather than being a constant amount per child. If parents are motivated by the total value of transfers, then we would expect to see larger responses to the introduction of the PFD compared to the Quebecois and Israeli child subsidy programs. If, on the other hand, parents are impatient and have high discount factors

¹⁰ Dividends for minors under the age of 18 are distributed to the parent or legal guardian of the child. A child born in 1985 would receive over \$23,000 in inflation-adjusted PFD payments by 2002 (in 2000 dollars).

(perhaps due to credit constraints), or are averse to uncertainty in the payment stream, we would expect to see smaller responses.

Because the payments are a fixed amount per person, we would expect that the effects of the PFD would be greater for lower-income households, as the payments represent a much larger relative increase in income. We might also expect that effects would be greater for larger families, because they receive a larger increase in lifetime income, with additional payments for each existing child.¹¹ On the other hand, families with more children may have already completed their childbearing and would not be incentivized by the additional income to have more children, leading to an ambiguous effect of family size.

Because American childbearing behavior has changed a great deal over time, I identify the impact of the PFD by comparing the childbearing behavior of Alaskans to that of other Americans (who weren't beneficiaries of the PFD program) and observe changes in Alaskan childbearing relative to that of their peers. The primary assumption of this difference-in-differences methodology is that Alaskan childbearing would have evolved similarly to that of the rest of the country in the absence of the PFD. This is important, as the demographics of Alaska are fundamentally different from much of the rest of the United States.

Table 2.1 presents summary statistics from the 1980 and 1990 decennial censuses for Alaska, the rest of the United States, and the Mountain and Pacific census regions (exclusive of Alaska), which contain the most similar states to Alaska.¹² Alaska is different from the other

¹¹ A 10-yr old child in 1982 would receive \$8,011 over the next 8 years; a 1-yr old child would receive \$19,221 over the next 17 years (all values in non-discounted, inflation-adjusted 2000 dollars).

¹² The Mountain region includes Idaho, Montana, Wyoming, Nevada, Utah, Colorado, Arizona and New Mexico. The Pacific region includes Alaska, Washington, Oregon, California and Hawaii. All results were run using both the entire US and just Mountain/Pacific states as comparison groups. The point estimates using only the

states across many dimensions, but these differences persist over the time period I use for analysis, and so do not affect the validity of my analyses.

Table 2.1: Summary Statistics of Selected Variables

	1980			1990		
	Alaska	Rest of US	Mountain / Pacific	Alaska	Rest of US	Mountain / Pacific
Age	26.91	33.45	32.55	29.02	34.84	33.60
Household Income (2000\$)	67,871	49,675	51,259	67,135	53,912	56,183
% White	77.8	85.6	87.1	75.7	80.4	75.9
% Black	3.5	11.8	5.3	4.0	12.0	5.3
% American Indian/Aleut	16.7	0.7	1.6	15.8	0.8	1.7
% Married (over 18)	67.9	65.5	64.3	64.6	61.2	60.1
% Employed (over 18)	66.7	60.6	62.2	70.6	63.8	65.0
Male % Less than HS grad	17.3	32.3	24.4	10.4	20.5	16.9
Male % HS grad	82.7	67.7	75.6	89.6	79.5	83.0
Male % College grad	22.4	20.0	23.5	23.2	23.2	25.7
% Female	48.5	52.9	51.8	48.5	52.5	51.2
Female % less than HS grad	17.6	33.7	26.0	11.2	21.3	17.9
Female % HS grad	82.4	66.3	74.0	88.8	78.7	82.1
Female % College grad	18.2	13.0	15.2	21.9	17.7	19.5
Children ever born	2.28	2.28	2.22	2.20	2.14	2.12
Age at first marriage	20.95	21.53	21.53			
Number of Observations	20,299	11,322,821	2,143,971	29,276	12,470,473	2,588,502

Notes: All data from 1980 and 1990 IPUMS from the decennial census. Mountain and Pacific estimates do not include Alaska. All estimates include population weights. Age at first marriage was not asked in the 1990 census.

Mountain/Pacific states are similar in magnitude, but with much larger standard errors due to smaller sample sizes.

Alaska is significantly younger than other states, with an average age almost 6 years younger than the rest of the U.S. Alaska also has much higher household incomes, though much of that is due to the increased cost of living in Alaska.¹³ Unsurprisingly, Alaska has fewer whites and blacks and more American Indians and Aleuts than the rest of the country. But Alaskans are also more likely to be employed, and both the population as a whole and the female population are more educated than the comparison groups. To account for these time-invariant population differences between Alaska and other states, I use a difference-in-differences methodology with state-specific linear trends that control for differing patterns of childbearing prior to the introduction of the PFD.

2.3 Data and Empirical Framework

Birth data come from the United States Vital Statistics Natality Detail Files from the National Center for Health Statistics (NCHS 2002) and include a near universe of all births occurring in the United States from 1972 to 2003. The Vital Statistics include information on the mother's race, ethnicity, state of residence and state of birth, as well as the child's birth order (the number of prior live births to the mother plus one).¹⁴ Births are classified according to the mother's reported state of residence. General fertility rates are calculated as the number of births to women residing in the state per 1,000 women of childbearing age (15-44) in that state. All population data come from the Integrated Public Use Microdata Series (IPUMS) of the 1970,

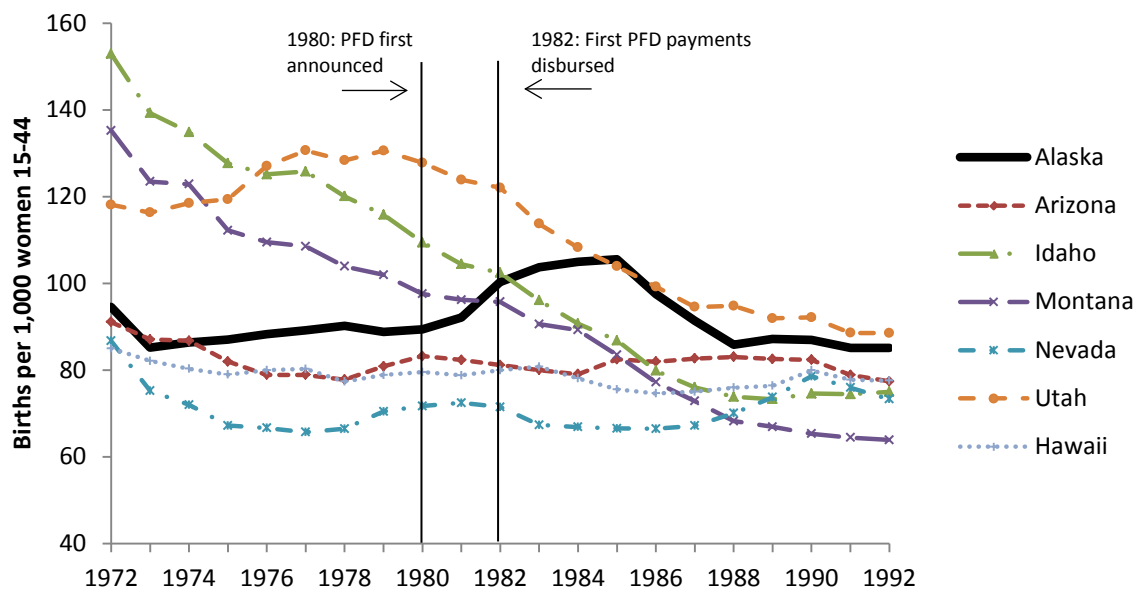
¹³ The cost of living for a family in Anchorage was 28% higher than the national average for an urban family in 1981 (Matthews 1981). And both food and housing costs are substantially higher in rural Alaska than in Anchorage; for example, food prices in Nome were 73% higher than in Anchorage in 1981 (Boucher 1994).

¹⁴ Information on mother's level of education is also included for some states, but is missing for over 20% of births through 1988.

1980, and 1990 decennial censuses and are linearly interpolated between censuses (Ruggles et al. 2015).

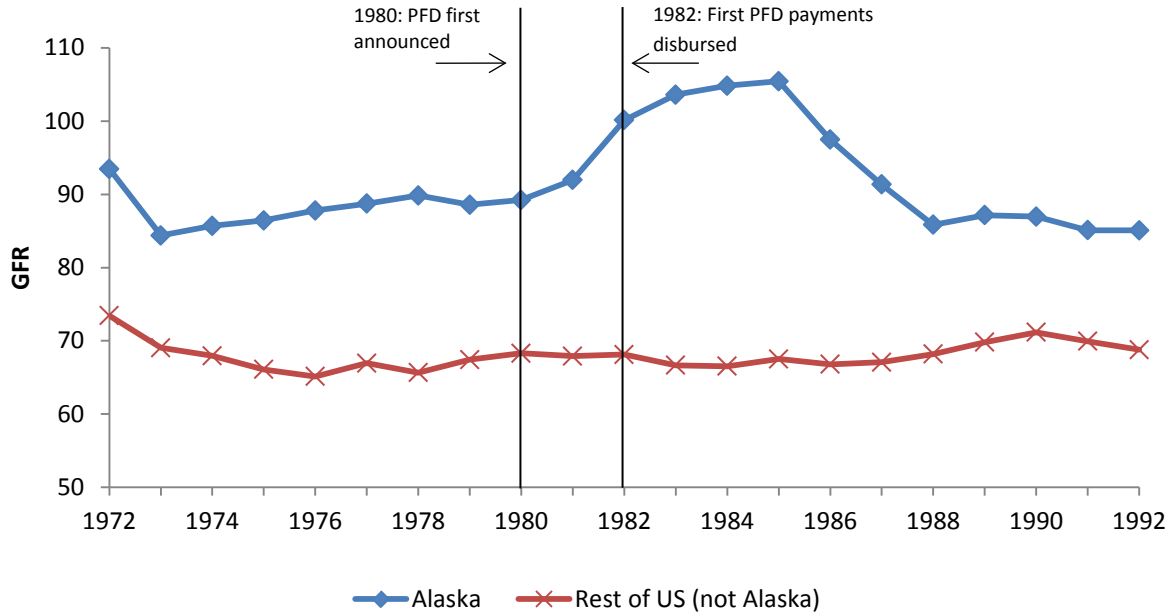
Figure 2.2a shows the general fertility rate for a subset of Pacific and Mountain states that are geographically or demographically close to Alaska. Figure 2.2b plots the general fertility rate for Alaska and the rest of the United States. It is clear that Alaskan fertility behaves differently from other states between 1981 and 1989, increasing from 92 births per 1,000 women in 1981 to over 105 births per 1,000 women in 1985, a 14 percent increase in the fertility rate, or almost a quarter of the increase experienced during the Baby Boom in Alaska.¹⁵ I hypothesize that this difference is due to the introduction of the Permanent Fund Dividend payments.

Figure 2.2a: General Fertility Rate Over Time for Alaska and a Subset of Pacific/Mountain States



¹⁵ Because Alaska did not become a state until 1959, early reports of vital statistics are difficult to find. The Alaska Department of Labor and Workforce Development reports that the crude birth rate for 1945 (the earliest available data) was 2.26, and the crude birth rate peaked in 1957 at 3.59, representing a 59 percent increase over the course of the Baby Boom. (2013)

Figure 2.2b: General Fertility Rate Over Time for Alaska and Other States



Notes: The general fertility rate (GFR) is the number of births per 1000 resident women ages 15 to 44 in a given state and year. The birth numbers come from Vital Statistics, the population counts from the 1970-2000 IPUMS of the Decennial Censuses.

To examine the differences in Alaskan fertility over time more closely, I use linear regressions of the following form:

$$Y_{st} = \alpha + \beta AK_s + \sum_t \gamma_t AK_s d_t + \delta d_t + \kappa_s t + \mu X_{st} + \varepsilon_{st} \quad (2.1)$$

where Y_{st} is the fertility rate (or other outcome of interest) of state s in year $t=1972\dots1991$; AK_s is a dummy variable equal to 1 if the state is Alaska and 0 otherwise; d_t is a set of year dummies; $\kappa_s t$ is a set of state-specific linear time trends; and X_{st} is a set of state-by-year demographic controls, including poverty rate, unemployment rate,¹⁶ the percent of the population that is nonwhite, the percent of the population over 18 with at least 12 years of

¹⁶ State-level unemployment rates are included from 1977 forward, but state-level unemployment data is not available prior to 1977, so I use state-group-level unemployment rates for 1972-1976. The state groups including Alaska consist of Alaska, Washington, and Hawaii from 1970-1972, and Alaska, Washington, Oregon, and Hawaii from 1973-1976. From 1968-1972, 32 states cannot be separately identified in the CPS, and from 1973-1976, 38 states cannot be separately identified.

school, the percent of the population over 25 with at least 4 years of college, and the percent living on a farm.¹⁷ The coefficient of interest, γ_t , represents the change in the difference in the fertility rate between Alaska and the rest of the United States relative to the omitted reference year, 1972. The estimates of γ_t test whether Alaskan fertility evolved in parallel to that of the rest of the United States, or if it diverged following the introduction of the PFD. Heteroskedasticity-robust standard errors are clustered at the state level (Arellano 1987).

This identification strategy relies on the assumption that in the absence of the PFD payments, Alaskan fertility would have evolved similarly to that of the rest of the country. This is plausible, because as Figure 2.2b shows, the trends in the general fertility rate were roughly parallel in Alaska and the rest of the U.S. prior to the PFD. One additional dimension that could invalidate this strategy would be time-varying differential access contraception. Using 1972 as a reference year removes many concerns with the diffusion of the birth control pill, as the Pill was legally available throughout the United States by that point.

2.4 Results

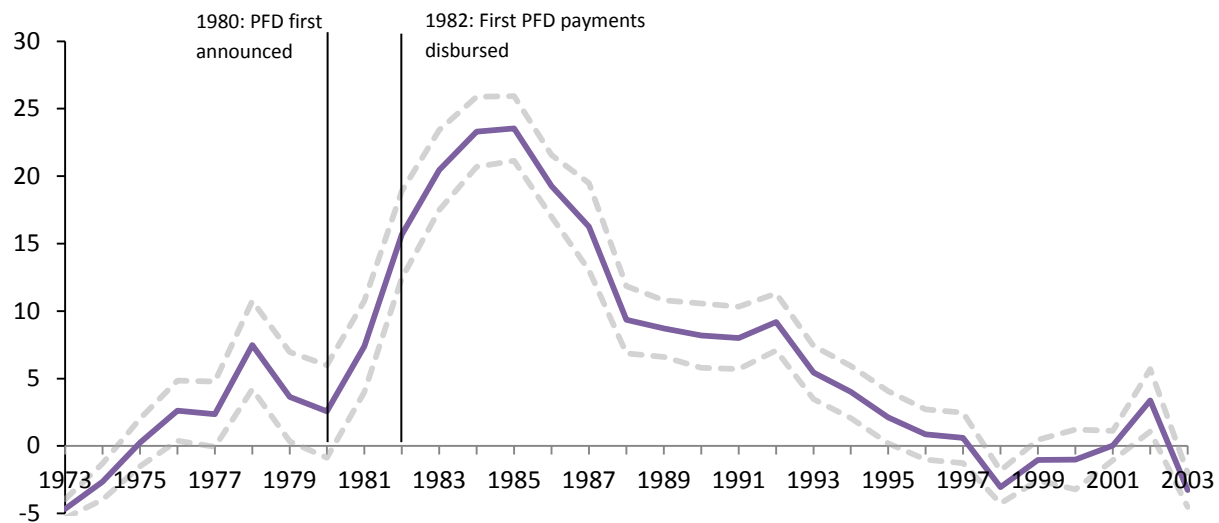
2.4.1 Effects on Period Fertility

Figure 2.3 displays estimates of the γ_t coefficients from regression (2.1) with the state-level general fertility rate (defined as births per 1,000 women age 15-44) as the outcome variable. These estimates represent the change in the difference between the Alaskan general

¹⁷ All controls except for unemployment rate are calculated from the decennial census IPUMS (Ruggles et al. 2015) and linearly interpolated between censuses. Unemployment rate comes from the March CPS (Flood et al. 2015). Additional state-level covariates obtained from Moody's Data Buffet, including real per capita disposable income and total retail sales, were also examined but did not substantively change the estimates.

fertility rate and that of the rest of the United States. Positive estimates indicate that Alaskan women increased their fertility relative to women in the rest of the U.S. (negative estimates indicate a relative reduction in Alaskan fertility).

Figure 2.3: Differential Evolution of General Fertility Rates (Alaska - rest of US)



Notes: The values shown are the Y_t estimates from regression (2.1) with the GFR as the dependent variable, and represent the change in the difference between the Alaskan General Fertility Rate and that of the rest of the United States, relative to 1972, along with 95% confidence intervals. Controls include poverty rate, % population over 18 with a HS diploma, % population over 24 with college degree, % nonwhite, % living on a farm, a state-specific linear trend and state-level unemployment rates.

The results support the hypothesis that a combination of exogenous positive income and negative price shocks increases fertility, at least in the short term. With the exception of a small jump in 1978, Alaskan women's fertility was slowly increasing at rate of around 1 additional birth per 1,000 women per year relative to the rest of the United States from 1973 to 1980, with an average of around 2 additional births per 1,000 women. Between 1982 and 1985, that trend dramatically accelerates, increasing at a rate of 4 to 7 additional births per year and peaking at nearly 24 additional births per 1,000 women in 1985, an increase of more than 25 percent over Alaska's baseline 1980 fertility rate of 89, roughly equivalent to half the size of the baby boom. Immediately after the peak in 1985, however, the effect diminishes rapidly,

dropping to 8 additional births by 1991, and to 2 additional births by 1995. The PFD appears to have increased fertility by an average of 12.4 births per thousand women (a 13.8 percent increase over the 1980 baseline) for a roughly fourteen year period, with the effect split into a larger initial effect for the seven years from 1982 to 1988, followed by a smaller sustained effect over the next seven years from 1989 to 1995. In this framework, the initial effect increased fertility by an average of 18.3 births per thousand women, or 20.3 percent, and the sustained effect increased fertility by an average of 6.5 births, or 7.3 percent.

One of the more interesting features of the increase in Alaskan fertility is that it appears to precede the PFD payments, at least slightly. The acceleration of Alaskan fertility in 1982 was driven by an increase in conceptions that almost certainly occurred prior to the distribution of PFD payments in June 1982 (children conceived in June 1982 would have most likely been born around March 1983). This suggests that Alaskan women anticipated the PFD payments in their childbearing decisions. This anticipatory effect is made more plausible because of the legal battles surrounding the PFD payments. It was clear from late 1980 that dividend payments would eventually be made to Alaskan citizens, it was just unclear until June 1982 exactly how much those payments would be and when they would be distributed. It is also possible that couples, knowing that payments were imminent and additional children would generate additional dividends, decided to accelerate their childbearing to take advantage of those additional dividends in advance of the actual payments.¹⁸ Evidence from Hsieh (2003) suggests that Alaskans do anticipate PFD payments in their purchasing decisions, smoothing their

¹⁸ Throughout this chapter, I assume that the household decision-making unit is a couple, though a substantial fraction of Alaskan and particularly native Alaskan fertility is non-marital (over 30% of Native American fertility was non-marital in 1980), in which case the household unit could be the mother alone.

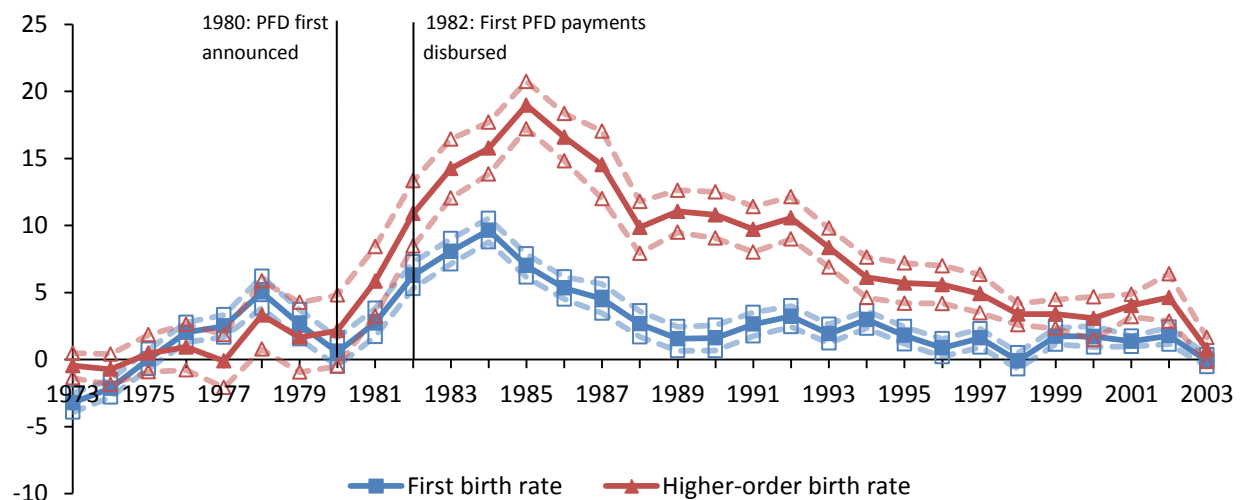
purchasing behavior across the year; however, it remains somewhat surprising that Alaskan families altered their childbearing behavior in response to the promise of future PFD payments.

It is also interesting that the large initial effect disappeared so quickly, as the PFD payments represented a permanent change in both income and the cost of children, as they were expected to continue in perpetuity and continued to rise after hitting their lowest level in 1984. This behavior is consistent with Easterlin's "relative income hypothesis", which posits that fertility is determined by the difference between a couple's "material aspirations and their resources—what might be termed the 'relative affluence' of the couple" (Easterlin 1976). In this framework, if a couple's perceived "earnings potential" is greater than their "material aspirations" they will have more children (as well as other consumption goods). Similarly, if their earning potential is less than their aspirations, they will be more hesitant to have children, irrespective of their actual level of income. Thus, the PFD payments would have increased the earning potential of couples whose material aspirations were set prior to the introduction of the PFD, resulting in increased births. For younger couples whose material aspirations included the PFD, however, the continued payments would have already been incorporated into their material aspirations and thus would not increase childbearing, because they would adjust their consumption of other durables instead. It appears as though Alaskan women initially accelerated their childbearing in response to the PFD payment announcement, but subsequent cohorts adjusted their expectations and consumption patterns, reverting to their pre-PFD childbearing behavior.

Because families with children would expect a larger income shock (as each child receives a PFD payment), we might expect to see larger impacts along the intensive margin of

second or higher births as opposed to the extensive margin of first births. To examine this in more detail, I calculate first and higher-order birth rates (i.e., the number of first or higher-order births per 1,000 women age 15-44). Figure 2.4 displays the γ_t coefficients with the state-level first birth rate and higher-order birth rates as the outcomes of interest.

Figure 2.4: Differential Evolution of First and Higher-Order Birth Rates (Alaska - rest of US)



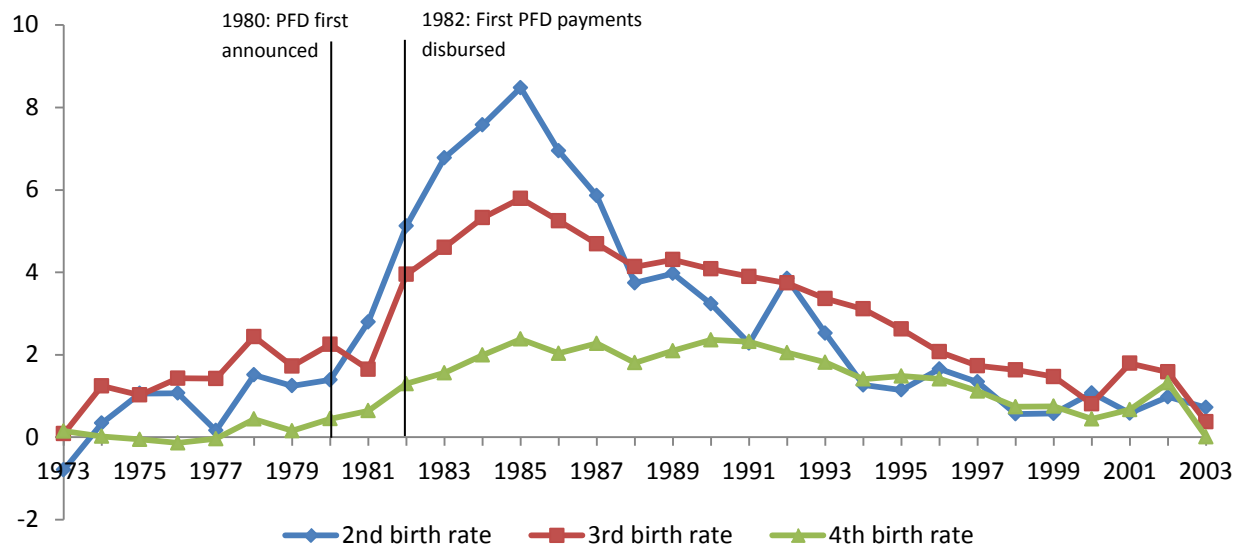
Notes: The values plotted are the γ_t estimates from regression (2.1) with either the first birth rate or the higher-order birth rate as the dependent variable, along with 95% confidence intervals. They again represent the change in the difference in the outcome of interest between Alaska and the rest of the United States, relative to 1972. The first/higher-order birth rate is defined as the number of first/higher-order births per 1000 women in a given state and year. Each series is a separate regression. All regressions include the full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

Figure 2.4 shows that while Alaskan first and higher-order birth rates follow a similar pattern relative to the rest of the U.S. prior to the PFD, the trends diverge sharply starting in 1982. While Alaskan first births do accelerate following the PFD, the effect is much stronger and longer lasting for higher-order births. The effect on first births peaks in 1984 at 9.6 additional births per thousand women, and drops to 1.6 additional births by 1990. The effect on higher-order births, however, peaks at 19 additional births in 1985, and remains above the pre-PFD peak of 3.3 additional births (1978) until 1999. Clearly, the effect of the PFD was much greater

and more persistent for higher-order births than first births. This finding is consistent with surveys of Italian women showing that the effect of cash transfers on fertility operated primarily on the intensive margin of second and particularly third births, rather than on the extensive margin of first births (Rondinelli et al. 2006).

Decomposing the higher-order birth rates further, into second, third, and fourth birth rates in Figure 2.5, I find that while all higher-order births differentially increased in Alaska relative to other states starting in 1982, the effect is strongest but shortest for second births. The effect of the PFD on second births rises sharply starting in 1982, peaking at 8.5 additional second births per thousand women in 1985, but then declines almost as sharply until 1988 when the downward trend slows, dropping below pre-PFD levels in 1994.

Figure 2.5: Differential Evolution of Higher-Order Birth Rates (Alaska - rest of US)



Notes: The values plotted are the Υ_t estimates from regression (2.1) with the 2nd, 3rd, or 4th birth rate as the dependent variable. They again represent the change in the difference in the outcome of interest between Alaska and the rest of the United States, relative to 1972. Each series is a separate regression, and all regressions include full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

The initial acceleration of the effect on third births is not as dramatic as that of second births, but the effect has a much slower decline, remaining above pre-PFD levels until 1997. Third and fourth birth rates are the only outcomes where the initial effect persists until 1990. Interestingly, the trend of fourth births was roughly constant in Alaska relative to the rest of the U.S. prior to the PFD, but from 1982 to 1995 Alaskan women had on average 1.92 additional fourth births. The persistence of the effects for third and fourth births, together with their higher parity, suggests that the PFD may have increased completed childbearing if those children were born to women who would not otherwise have had third or fourth children.

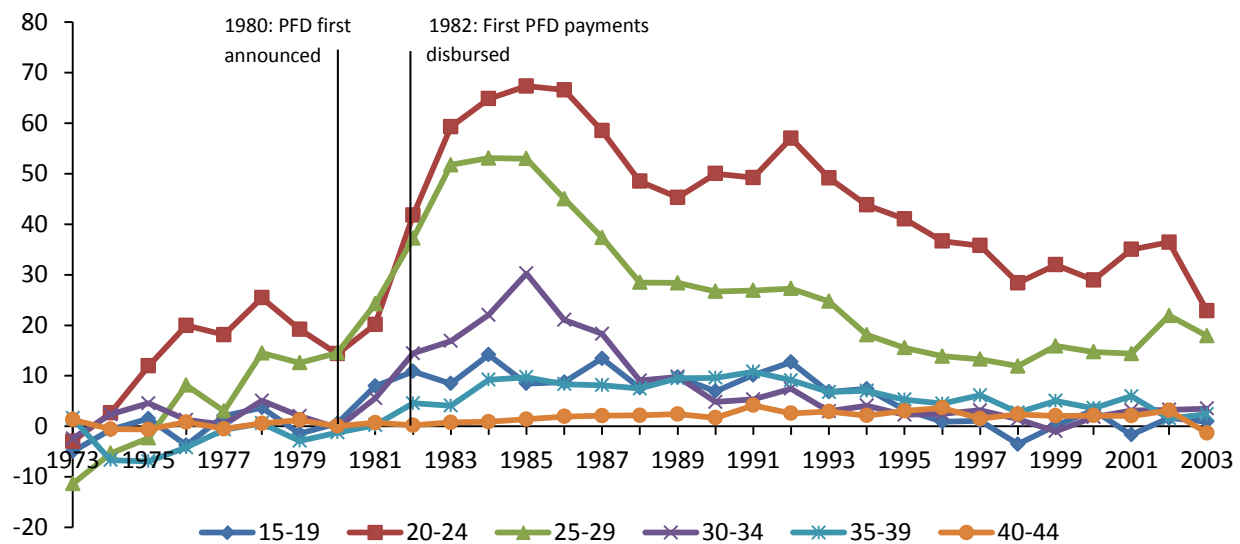
The larger magnitude of the initial effect on second births relative to third and fourth births runs counter to papers such as Milligan (2005) and standard economic theory, which suggest that the effect should be increasing in parity, as larger families would receive larger payments (one per family member). Additionally, unlike many other child subsidy programs such as those in Quebec or Israel, where payments increased for each additional child (up to 3 in Quebec, or 6 in Israel)¹⁹, the change in the cost of additional children from the PFD is linear rather than increasing in the number of children, as each additional Alaskan child obtains the same payment stream. The diminishing effect in parity could be because the quality-adjusted cost of children (in the Becker sense) is increasing in the number of children, or because families with more children are close to or at their optimal level of childbearing.

Figure 2.6 shows difference-in-differences estimates for age-specific fertility rates. These results show that young adult women (aged 20 to 29) reacted the most strongly to the

¹⁹ In Quebec, from 1992 to 1997, the benefits for a first child totaled C\$500; for a second child, C\$1000; and for a third child, C\$8,000 (Milligan 2005). In Israel, in 2000, first and second children received monthly payments of NIS 191, third children received NIS 381, fourth NIS 772, fifth NIS 648, and sixth or higher children received NIS 715 (Cohen et al. 2013).

PFD payments. This is consistent with the literature, which has shown that the bulk of changes to fertility behavior occur in those age ranges, even as far back as the Baby Boom (Bailey, Guldi, Hershbein 2013).

Figure 2.6: Differential Evolution of Age-Specific Fertility Rates (Alaska - rest of US)



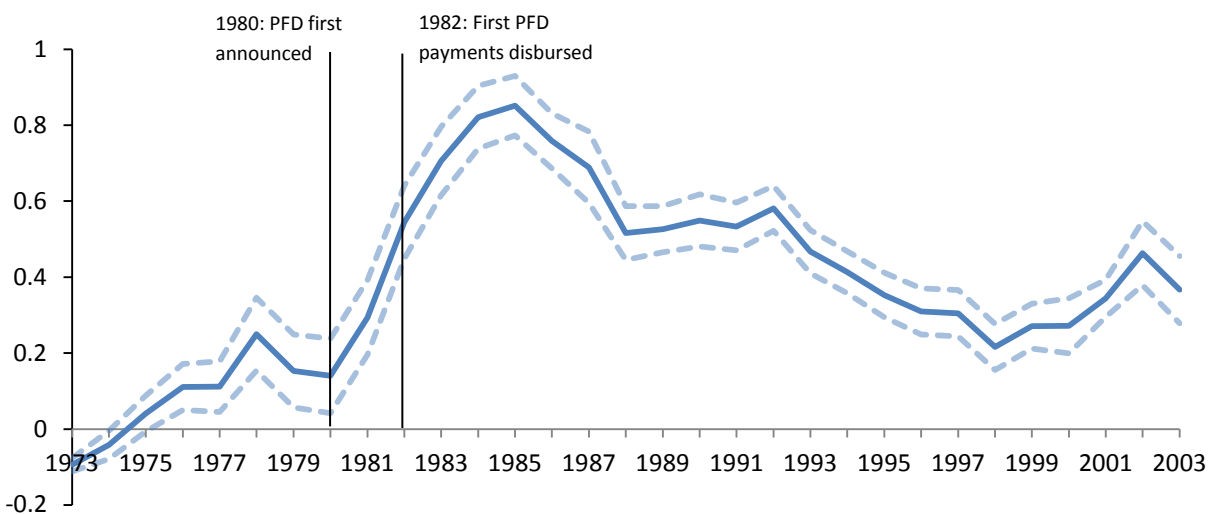
Notes: The values plotted are the Υ_t estimates from regression (2.1) with age-specific birth rate as the dependent variable; they represent the change in the difference in the outcome of interest between Alaska and the rest of the United States, relative to 1972. The age-specific birth rate is defined as the number of births per 1000 women in a given age group, state, and year. Each series is a separate regression. All regressions include the full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

The initial effect is strongest and longest for the 20- to 24-year-old age group, peaking at 67.3 additional children per thousand women in 1985, and remaining above the pre-PFD peak of 25.4 (in 1978) all the way until 2003. This is the most extended effect of the PFD found in any of the outcomes I examine. The effect for 25- to 29-year-old women is nearly as large as that for 20- to 24-year olds, but shorter lasting, peaking at 53.1 additional children per thousand women in 1984, dropping quickly to 28.5 additional children by 1988, and falling below pre-PFD levels in 1994. 30- to 34-year old women had a smaller, shorter, and slightly more delayed reaction, peaking at 30.2 additional children in 1985, and falling below pre-PFD levels in 1990. I

find small positive effects for teens (15- to 19-year olds) and older women (35- to 39-year olds), and no effect for the oldest age group (40- to 44-year olds). Interestingly, the small positive effect of increased family income on teen birth rates is in contrast with Lovenheim and Mumford (2013), who found that an increase in parental housing wealth, measured by home values, resulted in a drop in teen fertility. This is perhaps because the PFD also reduced the cost of children, or because teens anticipate an increase in their own income in addition to that of their parents, as the PFD payments will go directly to them once they turn 18.

Figure 2.7 combines the age-specific fertility rates into the total fertility rate (TFR). The TFR is defined as the sum of each of the age-specific fertility rates multiplied by 5 and divided by 1000, and represents the average number of children a woman would have if she faced all the age-specific fertility rates in that year.

Figure 2.7: Differential Evolution of Total Fertility Rates (Alaska - rest of US)



Notes: The values plotted are the $\hat{\gamma}_t$ estimates from regression (2.1) with the total fertility rate as the dependent variable; they represent the change in the difference in the TFR between Alaska and the rest of the United States, relative to 1972. The total fertility rate is defined as the sum of 5 times the age-specific fertility rates graphed in Figure 2.6, and represents the number of children a woman would be expected to have in her lifetime if she faced all of the age-specific fertility rates in that year. Each series is a separate regression. All regressions include the full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

Figure 2.7 shows that the initial effect of the PFD peaked at 0.85 additional children in 1985, resulting in an average increase in the Alaskan total fertility rate of 0.7 children between 1982 and 1988, roughly equivalent to the size of the baby boom. This initial effect represents an increase of 30 percent over the baseline Alaskan TFR of 2.34 in 1979. The sustained effect that followed from 1989 to 1995 increased the TFR by 0.49 children on average. The average increase in the TFR over the entire fourteen-year period from 1982 to 1995 is 0.59 children, or a 25 percent increase over the pre-PFD baseline. Interestingly, the TFR effect does not dissipate as quickly as the GFR effect, and in fact stays above the pre-PFD baseline until at least 2003, largely due to the sustained effects on 20- to 29-year old women.

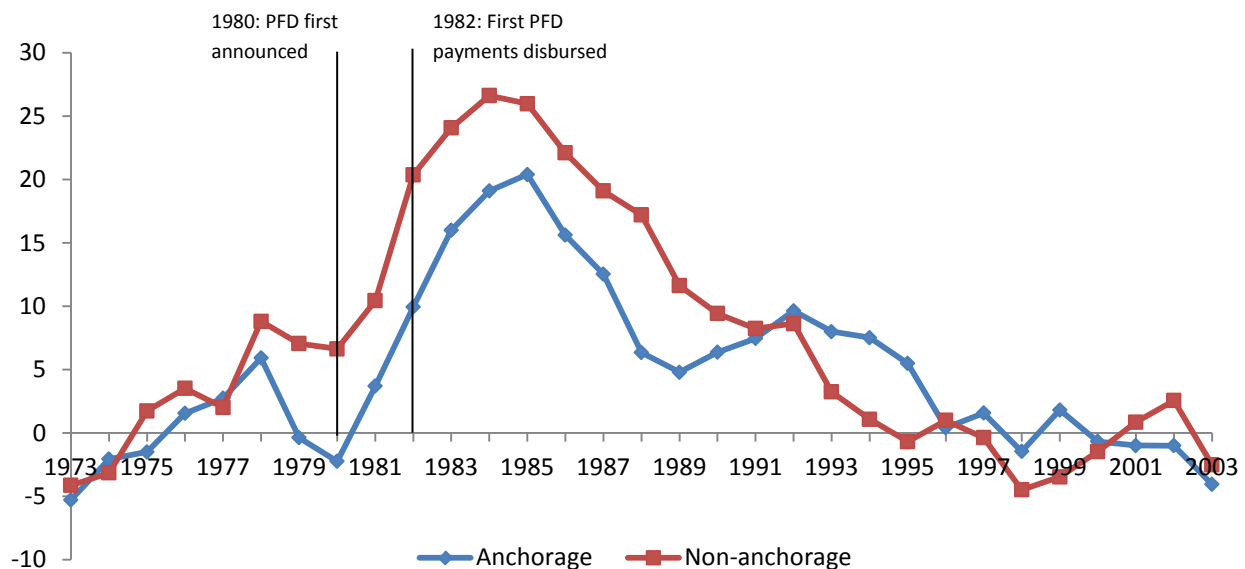
Because the PFD was a fixed amount per person, the relative income shock would have been much greater for lower income women. To examine this effect in more detail, I split births to women living in Anchorage from those living elsewhere in Alaska.²⁰ Anchorage is the largest population center of Alaska, containing over 45 percent of all Alaskan residents in 1980, and consistently has the highest median income of any borough in the state, so living in Anchorage is a reasonable proxy for income, particularly as women living outside of Anchorage in 1980 were more likely to be Native Alaskans and live a more subsistence lifestyle.²¹ To test the hypothesis that the effect of the PFD was stronger for lower-income, rural Alaskan women, I generate fertility rates for Anchorage equal to the number of births to women residing in the Borough of Anchorage divided by the population of women aged 15-44 in Anchorage from the Census multiplied by 1,000. Similarly, I generate fertility rates for the rest of Alaska, by

²⁰ Maternal education is the standard proxy for income, but education data is missing for over 20% of births in the Vital Statistics data through 1988.

²¹ Alaska uses boroughs instead of counties, and the Borough of Anchorage is the only independently identified borough in both the Vital Statistics and IPUMS for the entirety of my period of observation.

subtracting the Anchorage births from the total Alaskan births, and the Anchorage population from the total Alaskan population. For all other states, I use the regular GFR, and graph the difference-in-differences estimates in Figure 2.8.

Figure 2.8: Differential Evolution of Fertility Rates by Mother's County of Residence



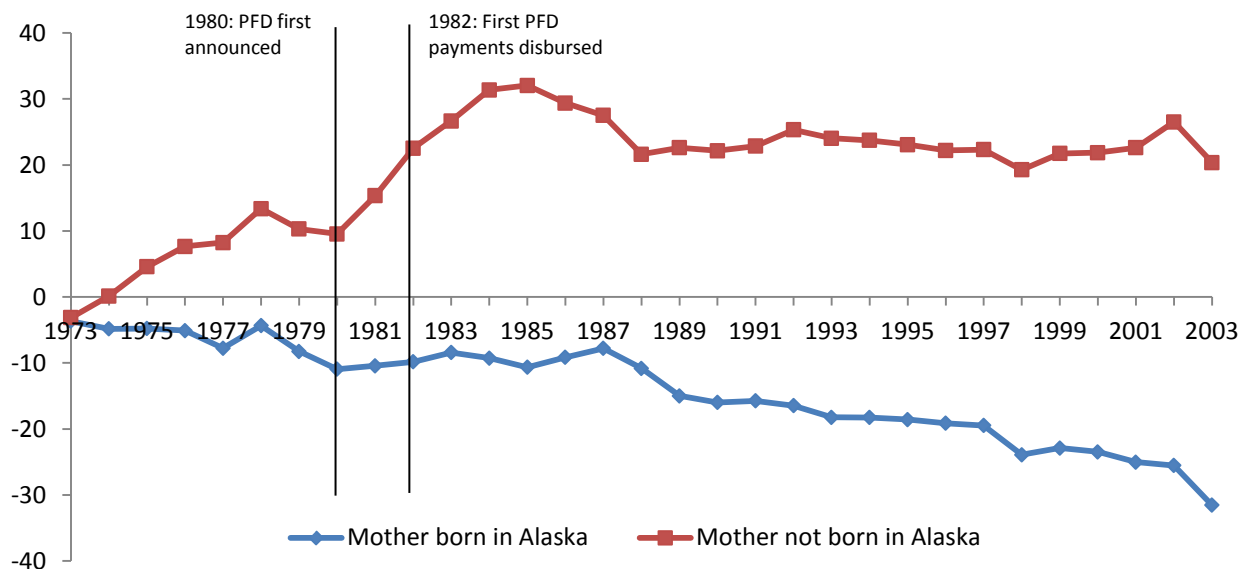
Notes: The values plotted are the Y_t estimates from regression (2.1). The dependent variable for both series is a slight modification of the general fertility rate in Figure 2.3. For all non-Alaskan states, the outcome variable is the GFR. For Alaska, the numerator is the number of births to either mothers residing in the Borough of Anchorage or mothers residing elsewhere in Alaska; the denominator is the population of women aged 15-44 living in or outside of the Borough of Anchorage. Each series is a separate regression. All regressions include the full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

I find that women living outside of Anchorage increased their childbearing by over five children per thousand women more than women living in Anchorage by 1985 (25.99 vs. 20.41), but non-Anchorage childbearing decreases faster after 1992, dropping below that of Anchorage. The initial impact of the PFD was greater for rural Alaskan women, suggesting that the effect is dependent on income and perhaps the opportunity cost of childbearing if women in Anchorage had better outside employment opportunities.

One hypothesis for the slight anticipatory effects of the PFD is that they were driven by women who had been residents of Alaska for many years and who would have anticipated

larger payments during the period between 1980 and 1982, when it was believed that the payment amount would be determined by length of Alaskan residency. Unfortunately, there is no information on residency length in the vital statistics, but there is information on state of birth.²² To test if long-term residents had a larger anticipatory response to the PFD, I split Alaskan births into those born to mothers who were born in Alaska and those born to mothers who were born outside of Alaska and generated rough fertility rates for both groups. In both cases, the denominator is simply the total number of Alaskan women 15-44. For all non-Alaskan states, I use the regular GFR.

Figure 2.9: Differential Evolution of Fertility Rates by Mother's Birthplace (Alaska - rest of US)



Notes: The values plotted are the Υ_t estimates from regression (2.1). The dependent variable for both series is a slight modification of the general fertility rate in Figure 2.3. For all non-Alaskan states, the outcome variable is the GFR. For Alaska, the numerator is the number of births to either mothers born in Alaska or mothers born not in Alaska; the denominator is the population of women aged 15-44 living in Alaska born in or outside of Alaska. Each series is a separate regression. All regressions include the full demographic controls, state-level unemployment rate, and a state-specific linear time trend.

²² Using state of residence 5 years prior in the census is problematic because I cannot test anticipatory responses from 1980 to 1982 in the 1980 census, and knowing if a woman lived in Alaska in 1985 in the 1990 census does not provide information about her residency status in 1982.

The results of regression (2.1) using the Alaskan/non-Alaskan mother birth rates are presented in Figure 2.9. They show that the change in fertility is driven largely by mothers who were born outside of Alaska. The samples, however, are not entirely comparable, as the mothers who were born in Alaska are much more likely to be of American Indian/Aleut descent and make up a minority of the overall population of Alaskan women. This test, however, does not provide any support for longer Alaskan residence, and therefore higher expected PFD payments prior to 1982, causing stronger anticipatory fertility effects.

2.4.2 Differential Migration

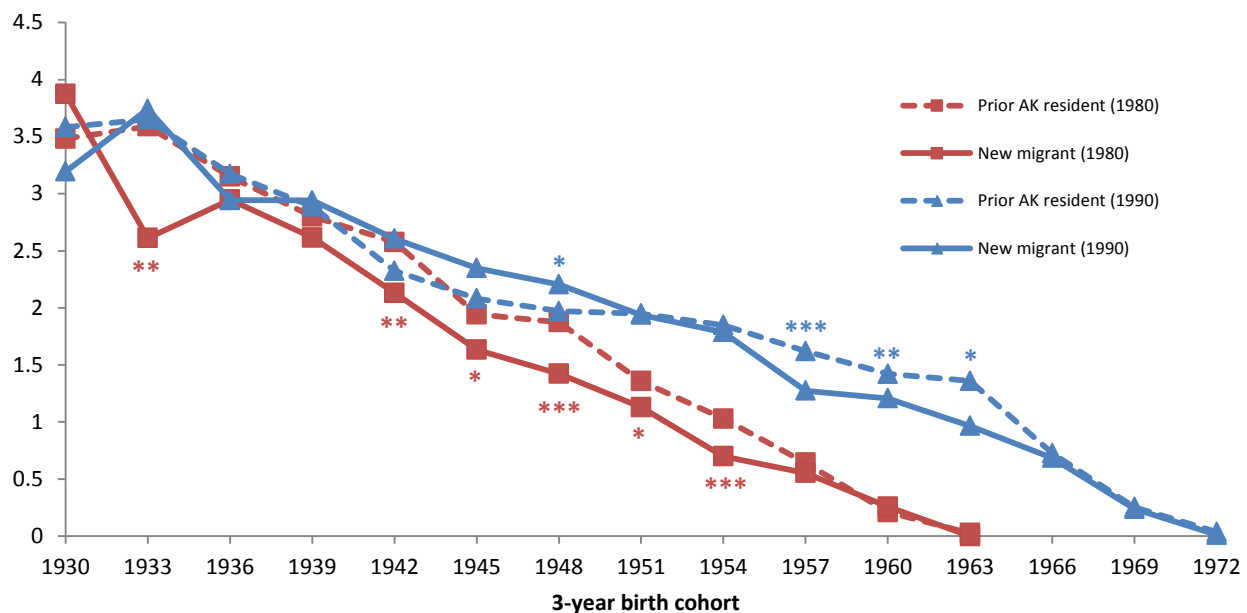
In order to interpret changes in the difference between Alaskan fertility and that of the rest of the U.S. as a behavioral response to the PFD, I need to be sure there were not simultaneous compositional changes; for example, if households with underlying preferences for more children flocked to Alaska to collect PFD payments. This is a particular issue given that the bulk of the effect is driven by women who were not born in Alaska.

A 1984 survey of over 1,000 Alaskan households suggested that the PFD had no immediate impact on in-migration and a negligible effect on out-migration. In that survey, no one responded that they had moved to Alaska to receive a dividend, and less than one percent said they did not leave the state in order to receive a dividend (Knapp et al. 1984). Over time, however, that pattern may have changed as the PFD program became more widely known outside of Alaska.

To examine this issue of selective migration explicitly, I compare completed fertility between new migrants and prior residents in Figure 2.10, using data from the 1980 and 1990

censuses (Ruggles et al. 2015).²³ I use children ever born rather than period fertility rates because completed fertility better captures overall preferences for children.²⁴ This measure has the disadvantage of including children born outside of Alaska, but provides a more informative comparison than attempting to recreate Alaskan total fertility rates from the census.²⁵

Figure 2.10: Alaskan Children Ever Born by Migration Status and Birth Cohort



Notes: The values plotted are the mean children ever born for Alaskan women in a given 3-year birth cohort, split by whether the woman reported living in Alaska 5 years prior to the 1980 or 1990 census. All values include the census population weights. * indicates that the difference between new migrants and prior residents is significant at the .1 level; ** indicates significance at the .05 level; *** indicates significance at the .01 level.

New migrants are defined as Alaskan residents who were not living in Alaska five years prior (prior residents reported living in Alaska five years ago). Native Alaskans do not provide a good comparison group because they are much more likely to be of Native American/Aleut

²³ In the 1980 census, only half the respondents were asked questions about migration, so the sample size is smaller for that year, but the comparison between new migrants and prior residents is unaffected.

²⁴ This is less true for younger women who likely have not finished their childbearing, and could capture a preference for earlier childbearing rather than a preference for larger families, particularly in women under 30.

²⁵ I generated these graphs, but the sample sizes are so small for children born in Alaska in a particular year who are still in an Alaskan household in the census year that splitting them by their mother's migration status yields very noisy estimates that are not particularly informative.

descent and thus have different fertility patterns. Alaska experienced a large influx of migrants starting in the early 1970s during the construction of the Trans-Alaskan Pipeline, and those migrants would not have been incented by the PFD.

In the 1980 census, women who lived in Alaska in five years ago (prior residents) had more children across all birth years than recent migrants, suggesting that migrants since 1975 did not prefer larger families. In the 1990 census, the pattern is largely the same, with the exception of the 1948-1950 birth cohort where new migrants had slightly more children than prior residents (in all other years the difference was not statistically different from zero, or showed prior residents having more children). Women born between 1948 and 1950 would have been 32 to 34 years old when the first dividend payments were disbursed, and thus outside of the age groups that showed the largest effects from the PFD (see Figure 2.7). The similarity of the differences between new migrants and prior residents in the 1980 and 1990 censuses provides suggestive evidence that the post-1985 migrants did not have differentially higher preferences for children than prior waves of Alaskan migrants. Together with the 1984 survey, these results suggest that the effects of the PFD on fertility were not driven by differential migration.

2.4.3 Effects on Completed Fertility

It is difficult to determine the impact of the PFD on completed fertility, due to data constraints, but it is possible that the difference-in-differences estimates presented in Figure 2.3 represent an increase in completed childbearing in Alaska, relative to the rest of the United States. There are two channels that can result in short-run fertility increases—women moving their childbearing earlier, or women having children they would not have had in the absence of

the PFD. Only the second would lead to an increase in completed fertility. If the effect were solely driven by women moving up their childbearing, we would expect to see fertility rates falling below pre-treatment levels (i.e., negative point estimates in Figure 2.3) following the surge in births from 1982 to 1989, but Alaska maintained consistently higher fertility than the rest of the country until 1998, when Alaskan women had 2 fewer births per 1,000 women than the rest of the country.

Since most women in Alaska had at least one child, an increase in first birth rates likely represents earlier childbearing, whereas an increase in higher order births could suggest that women were having children they might not otherwise have had. The sustained increase in Alaskan fertility in Figure 2.3, together with the sustained increases in third and fourth births in Figure 2.5 provide suggestive evidence that the PFD increased completed childbearing from 1982 to 1989, and maybe even until 1998.

2.4.4 Price Elasticity of Childbearing

A back-of-the envelope calculation can roughly estimate the implied elasticity of childbearing relative to the price of a child. Edwards (1981) provides estimates of the cost of raising a child from birth to age 18 by urban/rural status, region, and family income level. For Western non-farm children, her estimates range from \$52,211 to \$79,885 in 1980 dollars.²⁶ Because Alaska has a substantially higher average household income than the rest of the U.S. (Table 2.1), I use the upper bound of \$79,885 as my estimate of the cost of raising a child in Alaska in 1980.

²⁶ The estimates of total cost are the sum of the costs of raising a child at each age from <1 to 17 at three cost levels in 4 regions in 1980 dollars. The total cost is not discounted or adjusted for inflation. The Western region does not include costs for Alaska or Hawaii.

Calculating the reduction in the cost of a child is challenging, because the value of the 18 years of PFD payments was not known at the time that women were making their childbearing decisions. Unfortunately, I have no way of knowing how much money a woman in 1982 expected to receive from the PFD on behalf of her child. Instead, I calculate the total sum of payments a child born in 1982 would have received over the next 18 years and assume that women's expectations regarding the payment stream were similar. The sum of PFD payments from 1982 to 1999 total \$10,073 in 1980 dollars (\$16,547 in nominal dollars), which represents a 12.6% decrease in the cost of a child.²⁷ The initial effect of the PFD increased the general fertility rate by an average of 20.3% between 1982 and 1989. This suggests that the short-run price elasticity of childbearing is around -1.6 ($20.3/12.6$). This is larger than the elasticity reported in Cohen et al. (2003) of -0.54 following the introduction of child subsidies in Israel, but smaller than elasticities calculated in Malkova (2014) for paid family leave in Russia (-3.7) and Austria (-4.4), and child subsidies in Spain (-3.8) and Canada (-4.1)²⁸, suggesting that my estimate for Alaskan price elasticity of childbearing is within the range of other studies. Interestingly, the decreases in the cost of a child were much smaller in those studies (2.2% in Russia, 4.8% in Austria, 1.7% in Spain, 2.1% in Canada), but the Alaskan elasticity is only 35 to 45 percent of the elasticity estimates in those countries, perhaps indicating that the uncertainty around the value of the PFD payments dampened the response.

²⁷ For consistency with Edwards (1981), I use the non-discounted inflation-adjusted sum of payments in 1980 dollars. Using a 5% annual discount rate, the NPV of the cost of a child is \$50,444 and the NPV of the PFD payment stream from 1982-199 is \$6,301, so the PFD represents a 12.5% decrease in the discounted cost of a child.

²⁸ Elasticity calculations from Malkova (2014) using her own estimates for Russia and estimates from Lalive and Zweimuller (2009: Austria), Gonzales (2012: Spain), and Milligan (2005: Canada).

2.5 Conclusion

This paper examines the fertility effects of an exogenous non-wage income and child price shock, the introduction of the Alaska Permanent Fund Dividend (PFD) in 1982. I find that the PFD generated significant but short-lived increases in Alaskan fertility relative to the rest of the United States. The PFD increased the Alaskan total fertility rate by 0.59 children on average between 1982 and 1995, representing a 25 percent increase in total fertility, with the peak effect nearly the size of the baby boom. The effects are concentrated among second and higher-order births and young adult mothers, aged 20 to 29. The PFD could plausibly have increased completed fertility over that period, but data constraints limit the conclusions around completed childbearing. The relatively short duration of the general fertility rate effect supports the Easterlin relative income hypothesis, suggesting that subsequent cohorts of Alaskans incorporated the continued PFD payments into their expectations and material aspirations.

Consistent with previous literature, I find that the combination of the income and price effects from the PFD are substantial but largely fade over thirteen years, with the effects on the total fertility rate persisting longer than those on the general fertility rate. My results suggest that policies providing additional income to all citizens, including new children, should not be expected to have a meaningful long-run effect on childbearing.

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CHAPTER III

The Impact of Subsidized Birth Control for College Women: Evidence from the Deficit Reduction Act

3.1 Introduction

The birth control pill, or simply, the Pill, is the most popular form of contraception on college campuses in the United States. According to the American College Health Association, nearly 40 percent of college women use the Pill to prevent pregnancy, and of those women using any form of contraception, 61 percent use the Pill. In recent years, research has increasingly shown how availability of the Pill, especially during college, led to lasting impacts on women's human capital investments, career choices, and wages (Goldin and Katz 2002; Bailey 2006; Bailey, Hershbein, and Miller 2012). Yet, despite this importance, remarkably little is known about college women's sensitivity to price of the Pill, the extent to which they treat it as a substitute or complement to other forms of birth control, and how its use affects sexual behavior. With the newly implemented provision in the Affordable Care Act for prescription contraception with no co-pay, understanding the shape of college women's demand curve for the Pill is of considerable policy interest and importance.

This paper leverages a natural policy experiment to examine the effects of a dramatic change in the price of prescription birth control at college health clinics on contraceptive choice and sexual behavior. Prior to 2007, pharmaceutical companies sold prescription contraceptives

to college health clinics at deep discounts in order to attract brand loyalty among young consumers and receive tax deductions. As a result, students could obtain inexpensive birth control and colleges earned a bit of revenue by adding a small markup to help support other health initiatives around campus (Wasley 2007). This arrangement ended on January 1, 2007, when the Deficit Reduction Act of 2005 (DRA) went into effect, eliminating the incentives for pharmaceutical companies to sell drugs below retail price to all but a limited list of organizations, not including college health clinics. Consequently, the typical price of prescription birth control jumped from between \$5 and \$10 a month to between \$30 and \$50 a month on college campuses around the country. College health professionals worried that the change would lead to fewer women using the Pill, fewer well-woman visits (that could have lasting impacts on health), more use of emergency contraception, and more unintended pregnancies (Chaker 2007, Wasley 2007).

Exploiting the exogenous price change from the DRA and two complementary data sets on contraceptive choice and sexual behavior, we find that the policy reduced use of the Pill by *at least* 1.5 percentage points, or 3 to 4 percent, for college women overall. For college women who lacked health insurance or carried large credit card balances, the decline is two to three times as large. Additionally, we find small but significant decreases in the fraction of women who have had intercourse and the number of sex partners, suggesting that some women may have substituted away from sexual behavior in general. We also supplement these estimates with a unique survey on where college women actually obtain their birth control, allowing us to bound the price elasticity of demand of the Pill between -0.09 and -0.035.

The paper proceeds as follows. We first describe the history of price subsidies for

prescription medication at college health centers and the federal policy that ended these subsidies in 2007. The following section briefly reviews the experimental and quasi-experimental literature on the price sensitivity of birth control and family planning methods, highlighting the dearth of such research in a relatively affluent, developed country context. We then develop a simple theoretical framework that allows us to illustrate how the comparative statics from a price change might affect different margins of behavior (e.g., method of birth control, sexual frequency, partner status) and by magnitudes that vary across income levels. Sections 3.5 and 3.6 discuss the two datasets we employ in our empirical analysis, the National College Health Assessment and the National Survey of Family Growth, and the empirical strategies we use to identify the effects of the policy change. We next review our results, including several robustness checks to confirm the validity of our approach, before turning to our unique field survey of where college students obtain their prescription contraceptives, and how we use our estimates and these data to bound the price elasticity of the Pill for college students. Finally, we conclude.

3.2 Policy Background

President Bush signed the Deficit Reduction Act of 2005 (DRA) into law on February 8, 2006. The law, which went into effect January 1, 2007, was intended to reduce overall spending on Medicaid by reducing government payments for unnecessary services and cracking down on Medicaid rebate claims fraud. One of its provisions (Title VI, Subtitle A, Chapter 1, section 6001, part (d)) abridged the list of organizations that could receive “nominally” priced drugs from

pharmaceutical companies.²⁹ “Nominal” pricing allows the pharmaceutical companies to provide low cost drugs to clinics and organizations serving low-income populations without decreasing the “best price” paid by Medicare and Medicaid³⁰. Prior to the DRA, college health clinics were able to purchase contraceptives at the significantly lower nominal price, but since they were not explicitly named as eligible for nominal pricing in the Act, starting in January 2007, they were required to pay the full wholesale price for all drugs. This had the effect of raising the price of oral contraception on college campuses from around \$5 to \$10 for a month’s supply to between \$30 and \$50 (Rooney 2007). Because this specific provision was buried in the language of the bill, most college clinics did not hear of the change until an American College Health Association bulletin in December, 2006, less than a month before the law went into effect. Despite the late notice, many schools were able to stockpile reduced price contraceptives to sell to their students until supplies ran out. The University of Michigan, for example, was able to purchase enough to last until mid-2008, though other schools ran out much sooner (Rooney 2007). The effect of the Act went beyond just birth control prices; pharmacies at several college campuses, including Duke and Florida State, were forced to close after losing the revenue from their small markup over the nominal price on oral contraceptives (Cho and Reddy 2009).

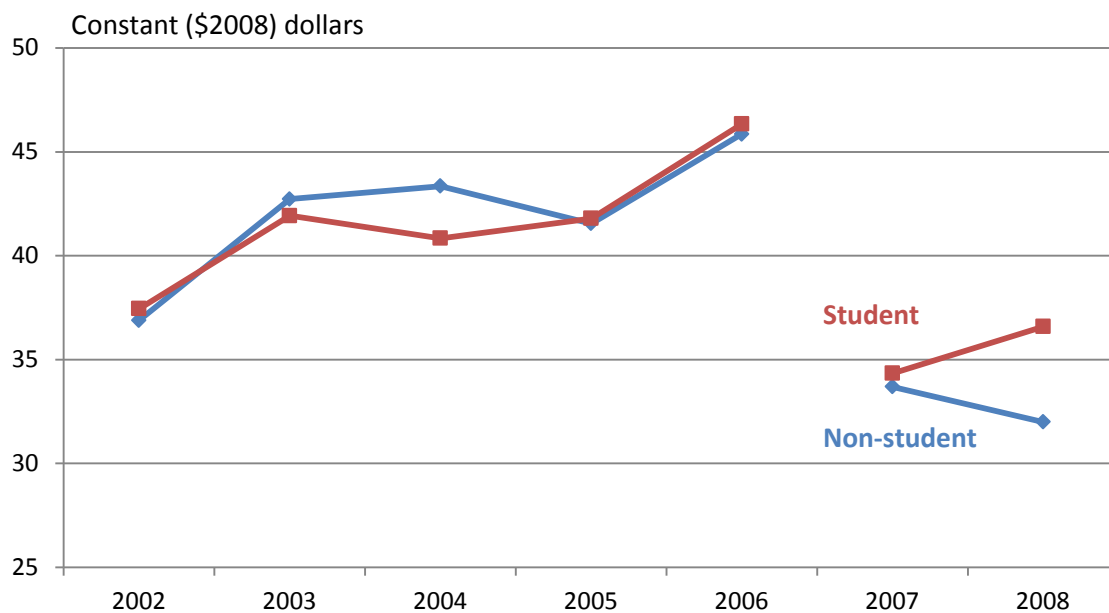
Importantly, this provision was both accidental and unexpected. Many legislators have said the omission of college health clinics was unintentional and a result of last minute word

²⁹ As part of the earlier Omnibus Budget Reconciliation Act of 1990, certain organizations—including non-profit health centers at college campuses—could buy prescription medications from pharmaceutical companies at a “nominal price,” without affecting the price Medicare and Medicaid paid for the drug.

³⁰ The “best price” is generally the lowest price offered to any commercial (non-governmental) customer, excluding nominal prices. This price is used to determine the manufacturer rebates owed to state Medicare and Medicaid agencies.

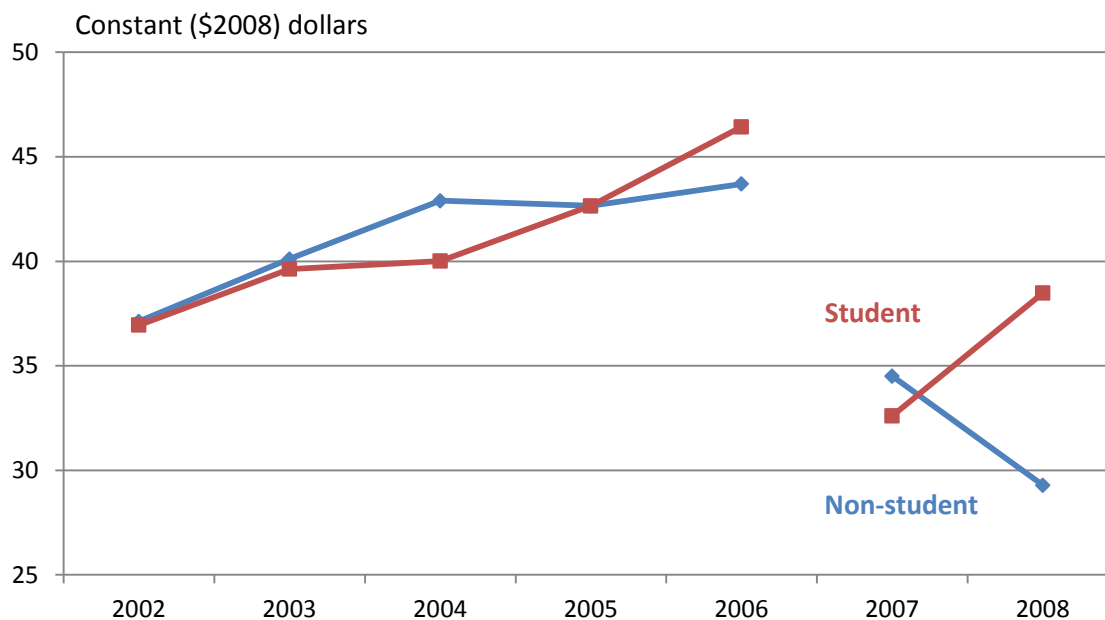
changes in conference committee (Davey 2007). Organizations that promote the health of college students and access to family planning such as the American College Health Association and Planned Parenthood were caught unaware and began lobbying efforts to reverse the provision only several months *after* it had gone into effect (Rooney 2007, Davey 2007). They were not successful until the “Affordable Birth Control Act” was introduced into the 2009 Omnibus Spending Bill and took effect in March of 2010. Consequently, prices for prescription drugs, including hormonal birth control, at college health centers were elevated for approximately a three-year period from 2007 to 2010.³¹

Figure 3.1a: Mean Birth Control Rx Prices Paid, per month



³¹ It is not clear how quickly or how many pharmaceutical firms reinstituted nominal pricing at college health centers after the law passed, so elevated pricing may have continued to a degree.

Figure 3.1b: Median Birth Control Rx Prices Paid, per month



NOTES: The outcome variable is the price paid by 18 to 29-year-old women for a 30-day supply of prescription birth control, excluding emergency contraception (i.e., the morning-after pill), separated by student status. There is a series break in 2007, due to a change in the method of data collection. SOURCE: Authors' calculations from the Medical Expenditure Panel Survey (MEPS).

The price change is clearly evident in the Medical Expenditure Panel Survey (MEPS), which collects detailed information from households about their medical expenses, including prescription birth control. Figures 3.1a and 3.1b provide the mean and median prices paid for prescription birth control, excluding emergency contraception, from 2002 to 2008 by student status. Though the prices track very closely until 2007, they diverge sharply in 2008, when the price students pay overtakes that of non-students.^{32,33}

³² The method of data collection changed between 2006 and 2007, resulting in a series break, but it should not have affected students and non-students differentially.

³³ Although the MEPS sample is not particularly large (especially for students), the difference in price is statistically significant at the 10 percent level only in 2008 for both the mean and median based on cluster-bootstrapped confidence intervals. Results are similar if we condition on oral contraception (thus excluding other prescription

The Deficit Reduction Act of 2005 provides a nearly ideal natural experiment through which to investigate college women's price sensitivity to the Pill, as it caused a large, exogenous increase in the cost of the Pill at campus health clinics. The magnitude of the shock, a greater than three-fold increase in price, is an important aspect of our research design for two reasons. First, much of the extant literature on the price elasticity of contraception, described below, has tended to find small or zero estimates for moderate price changes, so a large price shock might be necessary to find a sizable effect empirically. Second, because we do not directly observe in our data *where* college women obtain their method of birth control, our empirical design is *intention to treat* in that the price increase is expected to affect only a subset of women in our sample (those that receive prescription birth control from college health centers). A larger price change increases the power of our research design. Furthermore, because the price change was exogenous, it can be used to assess the impact of Pill price among college women on a variety of social and educational outcomes, including substitution to other, possibly less effective forms of birth control, and changes in sexual behavior.

3.3 Literature Review

Though the birth control pill is the leading method of contraception among young women in the United States, especially among women attending college,³⁴ few studies have

birth control such as the patch, ring, or injectable). That the difference occurs in 2008 and not in 2007 is likely due to stockpiling.

³⁴ According to the 2006–2008 National Survey of Family Growth, 21 percent of American women aged 15 to 24 were currently using the Pill; of women this age who were using any form of contraception, 50 percent used the Pill (Mosher and Jones 2010). In our sample of college women, 40 percent were currently using the Pill or had the last time they had intercourse, and of those using any form of contraception, 61 percent used the Pill.

examined the responsiveness of Pill use to changes in price in the U.S. setting. In part, this is because causal identification is typically difficult, with few sources of exogenous change. The literature consists mainly of studies conducted in developing countries. Some studies take a non-experimental, cross-sectional approach (Akin and Schwartz 1988, Schwartz et al. 1989) and find use of contraceptives, including the Pill, to be relatively insensitive to small changes in price. Jensen et al. (1994) finds similarly inelastic demand for contraceptives for their overall sample in Indonesia, but substantially more elastic demand among poor households, implying that price elasticities likely vary by income and credit constraints. All these studies, however, rely on self-reported price estimates and include travel and time costs and so may not reflect the effects of purely monetary price changes.

Others studies use an experimental approach, randomly altering the price of condoms and oral contraceptives (Gadalla 1980, Lewis 1986, Cernada 1982, Ciszewski and Harvey 1995, Harvey 1994, among others), and generally find contraceptives to be relatively price inelastic. Ciszewski and Harvey (1995), in particular, look at the effects of an increase in the prices of certain, socially marketed condoms and oral contraceptives in Bangladesh and find significant decreases in sales of those contraceptives. In their review, however, Janowitz and Bratt (1999) show increases in overall demand for both condoms and the Pill in the treatment regions following the price increase, suggesting that people had simply substituted other brands in place of the more expensive, socially marketed condoms and oral contraceptives. Little research has attempted to assess the substitution between different contraceptive choices following price changes (Matheny 2004); this constitutes a major gap in the literature, which this paper begins to address.

Other studies examine how legal access to the Pill in the late 1960s and early 1970s changed American women's fertility, human capital investment, and labor force decisions (Goldin and Katz 2002; Bailey 2006, 2010; Bailey, Hershbein, and Miller 2012), and show large, statistically significant differences in Pill use between states where access was legal and states where it was not. Since changes in the legality of the Pill can be considered changes in the economic cost of use, if not a change in monetary cost, these findings suggest that women who faced a higher cost to obtain the Pill were substantially less likely to use it.³⁵

In a more recent American context, Kearney and Levine (2009) examine the effect of increasing the income eligibility threshold for Medicaid services, which effectively reduced the price of family planning services for newly covered women, on contraceptive use. They find that new Medicaid eligibility reduced the probability of not using birth control at last intercourse by 5 percentage points among non-teens. This eligibility change, however, not only reduced the price of contraceptives, it also reduced the price of health services more generally, making it difficult to disentangle how much of the effect is due to a price change and how much is due to greater access to or knowledge about contraception. In contrast, the source of identification in this paper is a price change for a specific form of birth control—prescription-based birth control—that is independent of family planning or health services.

Additionally, Levine (2000) provides evidence that American teenagers change both their sexual behavior and birth control choices in response to changes in the price of pregnancy, measured by labor market conditions, AIDS incidence, welfare benefits, and abortion

³⁵ Such an economic cost could comprise the psychic cost of breaking the law, stigma for opposing a social norm, search costs to find a provider, or travel costs to reach providers in other states.

restrictions. While changes in the price of contraception are not the same as changes in the price of pregnancy, this suggests that the response of college women to the exogenous change in prescription contraceptive prices might be to alter their sexual habits in addition to or in place of changing their contraceptive strategy.

3.4 Theoretical Framework

To assess the impact of a change in the price of birth control on women's sexual behavior and contraceptive choice, we develop a flexible theoretical framework. Suppose that a woman has utility over a composite consumption good, x ; sexual pleasure, E ; and her risk of pregnancy, P , which is a bad. Sexual pleasure itself is a function of three choice variables: relationship status, $r = 1, 2, \dots, R$; frequency of sexual activity, $s = 1, 2, \dots, S$; and choice of birth control, $k = 1, 2, \dots, K$. Similarly, risk of pregnancy depends on two of these choice variables, s and k . The woman's utility can thus be expressed as $U = u(x, E(r, s, k), P(s, k))$.

We make the reasonable assumptions that $P(1, \cdot) = 0$, where $s = 1$ corresponds to no sexual intercourse (abstinence), and that $P(\cdot)$ is nondecreasing in s for any k ; that is, more frequent sexual activity cannot decrease the chance of pregnancy no matter what birth control method is chosen. Additionally, we assume birth control methods can be ranked in (ascending) order of effectiveness in reducing $P(\cdot)$ and that this ordering is independent of sexual frequency.³⁶ For $E(\cdot)$, we remain completely agnostic and allow the determinants of sexual pleasure to depend freely on the three inputs.³⁷

³⁶ See Trussell (2007).

³⁷ Since r , s , and k are each drawn from finite sets, there are $R \times S \times K$ possible values of E .

While the choice of relationship status and sexual frequency do not have a direct financial cost, the composite good and birth control choice do. Normalizing the price of x to unity, the woman's problem can be expressed as:

$$\text{Max}_{x,r,s,k} u(x, E(r, s, k), P(s, k)) \quad \text{s.t.} \quad x + p_k \leq M, \quad (3.1)$$

where M represents income and p_k is the cost of birth control method k . Substituting for x using the budget constraint yields

$$\text{Max}_{r,s,k} u(M - p_k, E(r, s, k), P(s, k)). \quad (3.2)$$

This formulation shows the tradeoff between more effective, and presumably more expensive, birth control and reduced consumption of the composite good. Since r , s , and k are discrete, a woman chooses relationship status r^* , sexual frequency s^* , and birth control option k^* such that

$$u(M - p_{k^*}, E(r^*, s^*, k^*), P(s^*, k^*)) \geq u(M - p_k, E(r, s, k), P(s, k)) \quad \forall r, s, k. \quad (3.3)$$

This framework has several characteristics of note. First, if the function $E(\cdot)$ varies across women according to taste and biology, it is possible for women with the same income and facing the same prices to optimally choose different relationship statuses, frequencies of sexual activity, and methods of birth control. Second, the price sensitivity of the optimal birth control choice depends on the marginal utility of the consumption good and income. In particular, if $u(\cdot)$ is concave in x , which is a standard assumption, then higher income women should be

less sensitive to price than lower income women.³⁸ Third, a change in the price of birth control method k influences not only the optimal choice of birth control but potentially that of relationship status and sexual frequency as well (and, indirectly, the outcomes of sexual pleasure and pregnancy risk.) This last point is complex and merits discussion.

For sufficiently small price changes that do not cause a woman to switch birth control methods, relationship status and sexual frequency should also remain unchanged. This follows immediately from the weak axiom of revealed preference. In this case, the woman suffers a utility loss from lower consumption of the composite consumption good but does not otherwise alter her behavior. For larger price changes such that a change in birth control method is optimal, the woman must compare the payoffs through $E(\cdot)$ and $P(\cdot)$ across the entire $r \times s \times k$ set. It is quite possible for the woman to find it optimal to switch birth control methods but maintain her relationship status and frequency of sex. On the other hand, a different birth control method may imply the original $\{r, s\}$ choices are no longer utility-maximizing and either or both may change. Without imposing more structure on the utility function, *how* they change is an open-ended empirical question.

Our focus on effective price increases for prescription birth control generally and the Pill specifically³⁹, permits us to make contextual comments. Since prescription methods are among the most effective in reducing the risk of pregnancy, women who switch methods are likely to choose less effective methods, increasing P unless s is reduced. In turn, if s is reduced, optimal

³⁸ A woman will switch birth control methods if and only if $F \equiv u(M - p_k, E(r, s, k), P(s, k)) - u(M - p_{k^*}, E(r^*, s^*, k^*), P(s^*, k^*)) > 0$ for some r, s , and $k \neq k^*$. Taking the cross-partial derivative with respect to M and p_{k^*} , however, yields $\frac{\partial^2 F}{\partial p_{k^*} \partial M} = u_{xx}$.

³⁹ In our data, between 80 and 90 percent of college women using prescription birth control use the Pill.

relationship status can also change. Under the additional assumptions that (a) sexual pleasure increases in sexual frequency more in a partnered relationship than when single, and (b) that there is a threshold in sexual frequency below which being single is preferred to being with a partner, lower sexual frequency may induce a switch away from partnered relationships. This is shown in the panels of Figure 3.2.

Figure 3.2a

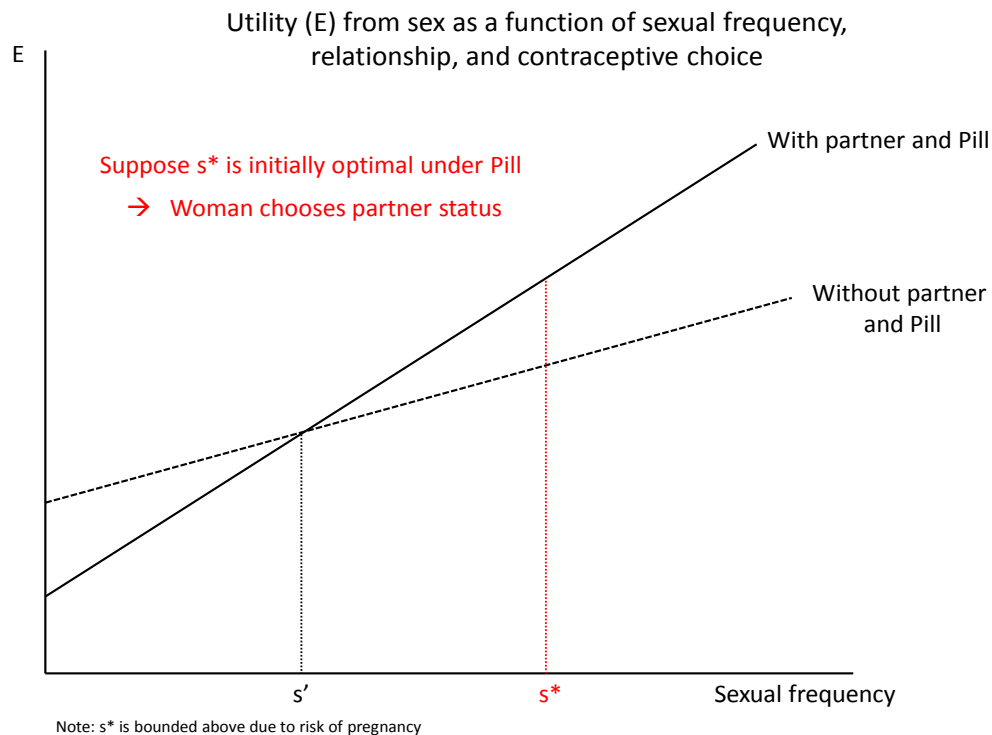


Figure 3.2b

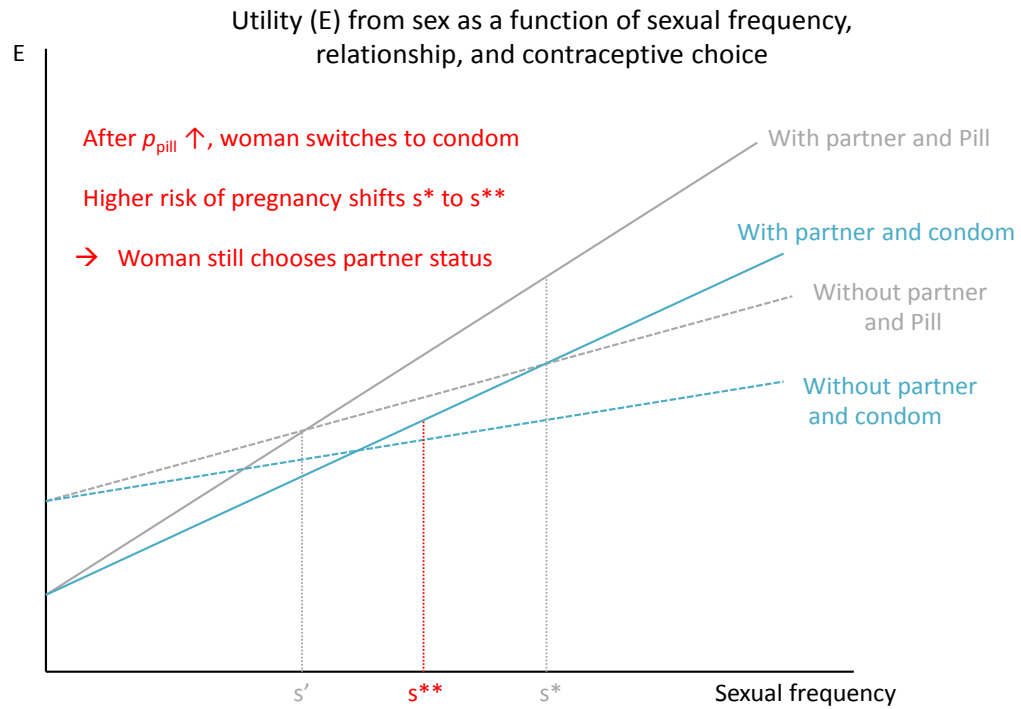
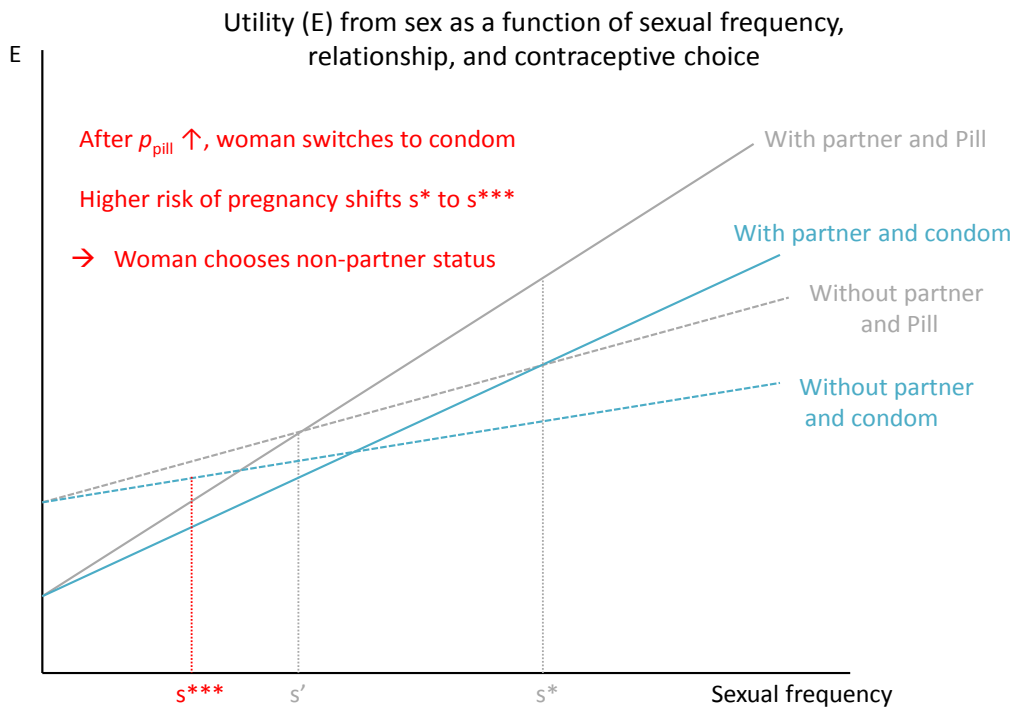


Figure 3.2c



Suppose a woman optimally chooses s^* , the Pill, and being partnered, as shown in the first panel.⁴⁰ If the price of the Pill rises and the woman finds switching to condoms as her next best method, she reduces her sexual frequency to s^{**} to offset the disutility from the higher risk of pregnancy of using condoms instead of the Pill. In the second panel, the woman still finds it optimal to be with a partner. However, if it is optimal for her to reduce her sexual frequency to s^{***} (the third panel), perhaps because she is more risk averse regarding pregnancy, she finds it best to be without a partner. The exercise demonstrates only that these behavioral changes are *possible* and worth investigating empirically.

3.5 Data Sources

We use two main sources of data for our analysis, the National College Health Assessment (NCHA) and the National Survey of Family Growth (NSFG). The NCHA is a large-scale survey of college students at participating colleges administered by the American College Health Association (ACHA). It has been conducted twice a year (the fall and spring semesters) since the spring of 2000 and asks a wide range of questions related to demographic characteristics, health, and risky behavior, including sexual behavior.⁴¹ In particular, the surveys include questions on the method of birth control used at last sex and sexual activity over the

⁴⁰ s^* is bounded above because of the risk of pregnancy.

⁴¹ We use the Spring 2000 through Spring 2009 survey waves for the majority of our analyses. Beginning in Fall 2008, a change in the survey instrument makes direct comparison with earlier waves somewhat challenging. In particular, the later instrument omits questions about credit card debt and shifts the time frame for questions about sexual activity from the last school year to the last 12 months. In addition, the later instrument changed the health insurance question from a binary “Do you have any kind of health insurance?” to “What is your primary source of health insurance?” Several other minor changes in wording include moving from “Did you use a condom the last time you had intercourse?” to “Did you use a method of birth control...” and changing the question about contraceptive methods from check all that apply with “Nothing” as an explicit answer to Yes/No radio buttons for each different method. Omitting responses from the later instrument does not substantively change the results.

last 30 days, as well as on a wide range of medical conditions, including sexually transmitted infections and pregnancy, and health-related academic difficulties over the last year. Between thirty and eighty colleges participate each semester, with many schools participating multiple times.

We restrict the data to include only 4-year colleges, as there are very few two-year colleges in the data and these colleges are much less likely to have full-service campus health clinics. Additionally, since not all institutions participate regularly in the NCHA, we restrict the sample to respondents at institutions that participated both before and after the policy change went into effect to reduce composition bias in our estimates. As institutions individually decide whether to participate in the survey (although the survey is administered to randomly selected students within schools), the NCHA is not nationally representative of all four-year colleges.

To explore the issue of representativeness, we compare our NCHA sample with four-year colleges from the Integrated Postsecondary Education Data System (IPEDS), a nearly complete universe of higher education institutions in the United States. Table 3.1 provides summary statistics from our NCHA sample, as well as the IPEDS sample and the subset of IPEDS restricted to only full-time students. The NCHA sample is younger than the full-time IPEDS sample, and substantially more white. NCHA also contains more schools from the West and fewer from the South and Northeast, as well as more large (over 10,000 students) schools than the IPEDS full-time sample. There are not significant differences in Pill usage by school size or by region, so we do not believe this poses significant analytical concerns.

Table 3.1: Comparison of NCHA Sample to IPEDS and IPEDS Full-time

	<i>NCHA</i>	<i>IPEDS</i>	<i>IPEDS: Full-time</i>
<i>Age</i>			
17-19	0.362	0.218	0.278
20-21	0.348	0.203	0.262
22-24	0.168	0.154	0.163
25-29	0.068	0.120	0.093
30+	0.045	0.202	0.110
Missing	0.010	0.104	0.093
<i>Race/Ethnicity</i>			
White	0.743	0.614	0.621
Black	0.056	0.122	0.116
Hispanic	0.054	0.093	0.091
Asian	0.096	0.054	0.059
Other/Missing	0.051	0.117	0.113
<i>International Student</i>	0.038	0.036	0.040
<i>Level</i>			
Undergraduate	0.882	0.773	0.846
Graduate/Professional	0.086	0.227	0.154
<i>Full-time</i>	0.952	0.717	1.000
<i>Region</i>			
Northeast	0.148	0.209	0.207
Midwest	0.230	0.244	0.240
South	0.237	0.330	0.327
West	0.319	0.196	0.205
<i>Public</i>	0.700	0.619	0.615
<i>Carnegie Classification</i>			
Baccalaureate	0.076	0.114	0.120
Masters	0.243	0.364	0.338
Doctoral	0.659	0.405	0.432
<i>Enrollment</i>			
< 2,500	0.046	0.133	0.140
2,500 – 4,999	0.038	0.125	0.122
5,000 – 9,999	0.144	0.173	0.163
10,000 – 19,999	0.380	0.235	0.229
20,000 +	0.393	0.335	0.345

Data shown represent female students attending 4-year colleges and universities over the 2000 through 2008 period. See text for information on the NCHA sample.

The NSFG is a nationally representative sample of women aged 15 to 44, only a small fraction of whom are currently enrolled in college. We use the 2006-2008 round of the NSFG, which has the unique and beneficial feature of being administered continuously from July of 2006 through December of 2008, thus spanning the policy change. The NSFG asks a wide range of questions related to fertility and family, including a retrospective contraceptive calendar that ascertains the contraceptive methods used each month over the 48 months preceding the interview. Importantly, though the NSFG contraceptive calendar questions are only asked of sexually active women who have ever used a contraceptive method, we adjust the universe to include those excluded women as non-Pill users to provide a more comparable universe to the NCHA. We define our treatment population to be women who are currently enrolled in school and whose highest level of education is at least part of one year of college.

We employ both data sets because they complement each other. The NCHA has the advantage of large sample sizes (on the order of 100,000 observations in most cases), providing sufficient power to detect small changes even among subgroups of women. However, because every woman in the sample is potentially affected by the policy change, there does not exist a natural control group other than college students before the policy change. We attempt to address this by comparing effects between large and small schools, as small schools are less likely to have on-campus pharmacies and are thus less likely to have been affected by the price change, but this is an imperfect control at best. The NSFG, on the other hand, allows comparison with similarly-aged women who were not college-goers and thus would not be affected by the policy change, and even allows within-person comparisons through the monthly contraception method calendar. However, its sample size of roughly 7,500 women, less than 20

percent of whom are attending college, comes with the cost of sharply reduced precision.

Additionally, unlike the NCHA, the NSFG does not identify the type of college women attend so we cannot restrict the sample to only 4-year colleges. Because the NSFG includes community colleges that typically do not have campus pharmacies, we would expect our results to be somewhat biased toward zero due to the inclusion of untreated women.

It is important to note that since we cannot identify where women obtain their birth control, we are employing an intention to treat (ITT) analysis and our estimates represent a lower bound of the effect of the policy change on Pill usage among women who were obtaining birth control from campus health clinics.⁴² This is because some women obtained birth control from other sources before the policy change and thus should not have been affected. Furthermore, even among women who were obtaining birth control from campus health clinics, the option to go elsewhere to obtain their contraception may mitigate against finding an impact. Planned Parenthood, in particular, is often used by college women, and the majority of their clinics were not affected by the DRA price increase.

3.6 Empirical Methodology

Because the two data sets admit different comparison groups, our empirical methodology differs somewhat between them. For the NCHA data, we employ interrupted

⁴² Information on where students obtain birth control is surprisingly scant. One article (Cottrell 2007) reported that 10 percent of female students at American University obtained prescription birth control at the Student Health Center in 2007. If we assume 40 percent of female AU students use the Pill, in line with the NCHA average, this implies that 25 percent of Pill users obtained their pills on campus. We have fielded our own survey of university students to obtain a more accurate estimate of where college students fill their birth control prescriptions, discussed in section 3.8.

time-series and (quasi-) differences-in-differences designs. For the NSFG, we employ differences-in-differences and fixed effects designs. These specifications are described in detail below.

For all approaches, we define our treatment period to begin in Fall (September) 2007. Our decision to begin the treatment period in the fall of 2007 reflects that the timing of the onset of the price increase varied by campus. Many colleges were able to stockpile nominally priced contraceptives in anticipation of the law, so that students were not faced with the retail prices until after those stocks ran out, typically in mid-2007 (Rooney 2007, Chaker 2007). Other college clinics had substantially smaller stocks, so the price increase took effect in early 2007. While this type of measurement error in the treatment period will tend to bias our results toward zero, we deal with this issue by omitting data from the first half of 2007, when treatment status is uncertain.⁴³

3.6.1 NCHA

We estimate the effects of the DRA using one of the following regressions

$$Y_{ijt} = \alpha + treat_t\beta + I_t + \mathbf{X}_{ijt}\lambda + f_j + \kappa t + \varepsilon_{ijt} \quad (3.4)$$

$$Y_{ijt} = \alpha + (treat_t \times D_j)\gamma + I_t + \mathbf{X}_{ijt}\lambda + f_j + \kappa t + \varepsilon_{ijt} \quad (3.5)$$

where Y_{ijt} is an outcome for person i at school j interviewed at time t , $treat_t$ is a dummy for our treatment period (equal to one if t is Fall 2007 or later and zero otherwise), I_t is an indicator for the survey instrument (Spring 2000 to Spring 2008 or Fall 2008 to Spring 2009), D_j is an indicator for whether a school is of a certain type, and \mathbf{X}_{ijt} is a vector of individual demographic

⁴³ Relaxing this restriction to include the Spring 2007 wave as treated does not substantively change the point estimates but increases the standard errors.

and behavioral covariates. The f_j terms denote fixed-effect dummies for schools and t is a linear time trend. Equation (3.4) is an interrupted time-series model, where the coefficient of interest, β , represents the deviation in trend in the outcome following the price change. Equation (3.5) modifies this model to allow the effect of the treatment to vary across groups, D_j . Functionally, we base D_j on the school's size: whether student enrollment is greater or less than 5,000. Since smaller schools are less likely to have a campus pharmacy, there is less of a chance that students there are effectively "treated," making these schools a plausible comparison group. The coefficient vector γ thus represents the effect of the treatment relative to a base group (a "dosage" effect), and for this reason we call the specification in (3.5) quasi-differences-in-differences. These estimates represent a lower bound on the true treatment effect on the treated, since not all students were receiving their birth control from the college health clinic prior to the price change, and so not all students were subject to the "treatment" of the price change.

In some specifications, we include school-specific linear time trends ($f_j \times t$) to capture school characteristics that evolve over time and may have affected Pill use and other outcomes independently of the price increase, such as attitudes towards the Pill or trends in sexual behavior on campus. Because the later survey instrument (Fall 2008 and later) does not include questions about credit card debt, specifications including credit card debt only use data through Spring 2008 and drop the I_t instrument dummies.⁴⁴ To allow for correlation in the error term among students attending the same school, we cluster standard errors at the school level.

⁴⁴ Omitting responses from the later survey instrument does not substantively change the results for the other outcomes of interest, suggesting that the inclusion of the additional three survey waves adds little additional information.

3.6.2 NSFG

The NSFG data provide two avenues for examining the effects of the DRA using the panel sample from the contraceptive calendar. In this framework, an observation is now a person-month rather than a person, and the time index is not the interview month, but the month referenced in the contraceptive calendar. The definition of $college_{it}$ is now such that it equals one only for the months during which the woman is enrolled in college.⁴⁵ Because there are now multiple observations for each individual, we can employ both difference-in-difference and fixed effect models:

$$Y_{it} = \alpha + treat_t\beta + college_{it}\delta + (treat_t \times college_{it})\rho + \mathbf{X}_i\lambda + \kappa t + \varepsilon_{it} \quad (3.6)$$

$$Y_{it} = \alpha + treat_t\beta + college_{it}\delta + (treat_t \times college_{it})\rho + \mu_i + \kappa t + \varepsilon_{it}, \quad (3.7)$$

where Y_{it} is an outcome for person i interviewed in month t , $treat_t$ is a dummy for treatment status (equal to one if t is on or after September 2007 and zero otherwise), and all other variables are as previously defined. In (3.6) and (3.7), the coefficient of interest is the difference-in-difference estimate, ρ , on the interaction of treatment and college-going, which represents the change in the difference of the outcome (e.g. Pill use) for women enrolled in college, relative to non-college-going women. In (3.7), individual fixed effects, μ_i , replace the controls \mathbf{X}_{it} from before. For heterogeneous effects, we run (3.6) and (3.7) on different subsamples of the data rather than impose additional interactions for ease of interpretation. For all NSFG models, standard errors are clustered at the PSU level.

Although all regressions with binary outcomes are run as linear probability models

⁴⁵ We determine that a woman was enrolled in college in month t if (a) she was enrolled as of the interview month, and (b) month t falls after she began college, assuming continuous enrollment. (That is, the interview month, less twelve times the number of years of college she had attained, falls before month t .)

(using OLS), the results presented below are robust to specifying probit models and calculating average partial effects, as well.

3.7 Results

3.7.1 Pill Use

Panel A of Table 3.2a presents the β coefficients on the treatment dummy from (3.4) with Pill use as the dependent variable, along with mean Pill use for reference. We have made the conscious decision to code women who are not using the Pill and are not sexually active as 0, identical to women who are sexually active and not using the Pill, because the theoretical framework makes clear that sexual activity may be an important margin of response, which we explore explicitly below.⁴⁶ We run regression (3.4) without any individual-level controls and with demographic controls (dummies for age, race, class, health insurance status, full-time student status, residence type, greek status, and credit card debt), both with and without school-specific linear time trends. We restrict the sample to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007 to reduce composition bias caused by colleges that participated only once or twice. Additionally, we omit the Spring 2007 wave because of its uncertain treatment status.

⁴⁶ Restricting the sample to sexually active women and including demographic controls yields point estimates that are very similar to those for the entire population but with generally larger standard errors.

Table 3.2a: NCHA – Pill Use at Last Sex

	(1)	(2)	(3)	(4)
A: All College Women				
Mean DV = 0.409 n = 95,886	-0.0223*** [0.0081]	-0.0166*** [0.0061]	-0.0248*** [0.0084]	-0.0172*** [0.0058]
B: by School Size				
Large (5,000+) Mean DV = 0.410 n = 87,860	-0.0242*** [0.0085]	-0.0184*** [0.0064]	-0.0259*** [0.0088]	-0.0179*** [0.0061]
Small (< 5,000) Mean DV = 0.394 n = 8,026	-0.0033 [0.0129]	-0.0004 [0.0076]	-0.0021 [0.0174]	-0.0050 [0.0190]
C: by School Size, time FE				
Treat x Large	-0.0057 [0.0130]	-0.0150* [0.0081]	-0.0252 [0.0212]	-0.0173 [0.0126]
Demographic Controls		X		X
School-specific Time Trend			X	X

Notes: Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex; women who have not had sex are coded as a 0. Each panel-column is a separate regression. Sample is restricted to schools that participated in at least two NCHA surveys prior to Spring 2007 and at least one after Spring 2007. Responses from both survey instruments are included (Spring 2000 to Fall 2009); Spring 2007 is omitted. All specifications include school dummies. Demographic controls include dummies for season, age, race, class, health insurance status, credit card debt, full-time student status, residence type, and greek status. * indicates $p < 0.1$, ** that $p < 0.05$, and *** that $p < 0.01$.

We find consistently negative and statistically significant estimates of the change in Pill usage following the price change. These estimates imply that overall women reduced their use of the Pill by 1.7 to 2.5 percentage points, or 4 to 6 percent, in response to the price increase. Across the columns in panel A, the treatment effect is remarkably consistent whether we include demographic controls or allow for school-specific time trends. The interrupted time series design in panel A does not provide the strongest identification, and it is possible an unobservable factor other than the price change is driving the result. Thus, panel B implements the specification in (3.5), where we allow the effect to vary by school size. Since very few small schools have campus pharmacies, we would expect the treatment effect to be much smaller at

these schools. And, indeed, that is what we find. At large schools, those with enrollment greater than 5,000 students, the coefficient estimates are slightly larger than those in panel A; for small schools, on the other hand, the point estimates are uniformly close to 0, though the standard errors are larger due to the substantially smaller sample size.^{47,48} An even stronger test of identification is presented in panel C, which replaces the linear time trend with a full set of time dummies. In this specification, we can no longer separately identify treatment effects for both large and small schools but can instead estimate only their difference. These point estimates are quite close to the implicit differences in panel B in all but the first specification, which is reassuring that linear trends do an adequate job of capturing secular variation.

While the NCHA data imply that the price changes induced by the DRA led to a small but significant reduction in Pill use among college women, the NSFG sample provides additional verification. Table 3.2b shows results from estimation of (3.6) and (3.7). The difference-in-difference estimate in the first row shows a 3.7 percentage point drop in Pill use among college women relative to other women, while the fixed effects estimate is a 1.3 percentage point drop. Although noisy, given the much smaller sample sizes, these results are quite close to the NCHA numbers, as would be expected if non-college women were unaffected by a price change at college pharmacies. The second panel restricts the analysis to college women in order to align more closely with the identification strategy in the NCHA. The point estimate of -0.0170 in the first column is nearly identical to the corresponding estimate in Table 3.2a (panel A, column

⁴⁷ Students at large schools are about 1.6 percentage points more likely to use the Pill than their counterparts at smaller schools, but this difference is accounted for by differences in observable demographics. Also, the assumption of common trending between school sizes cannot be rejected at 10 percent once demographic controls are included.

⁴⁸ The difference between school sizes is significant at 5 percent in the second column.

2), as is the fixed effects estimate in the second column. That a different and independent data set yields the same point estimate under a similar identification strategy supports the validity of our results. In summary, the policy change led to a modest reduction in Pill use among all college women.

Table 3.2b: NSFG – Pill Use in Month

	(1)	(2)
A: All Women		
Mean DV = 0.184	-0.0368	-0.0130
N(obs) = 159,675	[0.0276]	[0.0204]
n(women) = 3,695		
B: Students Only		
Mean DV = 0.316	-0.0170	-0.0166
N(obs) = 14,637	[0.0272]	[0.0314]
n(women) = 576		
Demographic Controls or FE	D	FE

Notes: Standard errors, clustered at the PSU level, are in brackets. Dependent variable is Pill use in a given month. All regressions include population weights. Each cell is a separate regression that is based on a retrospective contraceptive calendar and restricts the sample to women who were interviewed during or after September 2007. Panel A presents difference-in-differences estimates between college women and non-college women. Panel B presents differences estimates for college women only. Demographic controls include dummies for age, education, race, insurance, and poverty status. * indicates $p < 0.1$, ** that $p < 0.05$, and *** that $p < 0.01$.

As mentioned earlier, however, not all college women are likely to have been equally affected by the price change. In particular, women with health insurance are unlikely to have been fully affected by the price increase. Many insurance plans cover birth control prescriptions, and while it may be less convenient to get birth control through private insurance, and there is an additional risk of parental disapproval if the claim goes on parental policies—which may dissuade some women from utilizing their insurance coverage (Chaker 2007, Cottrell 2007)—it remains an option for women with insurance. As a result, though the price of birth control is likely to have gone up for them (few insurance policies at the time covered prescription birth control with a \$5 co-pay), the price change was probably less severe

for women with insurance (Frey 2007). Women without health insurance, on the other hand, were more likely to face a binding price increase due to their much more limited set of low-cost birth control providers outside of the campus health center, primarily Planned Parenthood or Title X clinics, which may or may not have been available in the area. For these reasons, we would expect a more pronounced impact among women lacking health insurance. The results of regression (3.5) with Pill use as the dependent variable by insurance status are reported in Table 3.3a.⁴⁹

Table 3.3a: NCHA – Pill Use by Insurance Status

	Has Health Insurance	No Health Insurance	Has Health Insurance	No Health Insurance
A: All College Women				
Mean DV HI = 0.420	-0.0137**	-0.0311***	-0.0143**	-0.0302***
Mean DV NHI = 0.349	[0.0059]	[0.0105]	[0.0056]	[0.0104]
n = 95,886				
B: by School Size				
Large (5,000+)				
Mean DV HI = 0.422	-0.0156**	-0.0317***	-0.0149**	-0.0301***
Mean DV NHI = 0.350	[0.0062]	[0.0106]	[0.0058]	[0.0106]
n = 87,860				
Small (< 5,000)				
Mean DV HI = 0.400	0.0028	-0.0300	-0.0031	-0.0290
Mean DV NHI = 0.332	[0.0091]	[0.0388]	[0.0221]	[0.0423]
n = 8,026				
C: by School Size, time FE				
Treat x Large	-0.0151	-0.0023	-0.0165	-0.0014
	[0.0092]	[0.0402]	[0.0180]	[0.0367]
School-specific Time Trend			X	X

Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex. All specifications include demographic controls. About 12 percent of the sample lacks health insurance, but this group is too small at smaller schools to be informative; in panel C, the restriction that the treatment effect is zero among students without health insurance at small schools is imposed to increase efficiency. See Table 3.2a for other notes.

⁴⁹ We classify women who report they don't know whether they have health insurance, a relatively small group, as not having health insurance.

We find that the reduction in Pill usage is over twice as large among women without health insurance relative to women with health insurance (about 3.1 percentage points, compared to 1.4 percentage points) and the larger negative effect for uninsured women is robust to the inclusion of school-specific linear time trends. Unsurprisingly, women with health insurance have significantly higher rates of Pill usage overall. These effects for insured women represent a roughly 3 percent decrease in the fraction of women using the Pill, while the effects for uninsured women represent a 9 percent decrease in the fraction of women using the Pill.

Looking at the results by school size, as in panel B, reveals similar but slightly larger effects at large schools. At small schools, on the other hand, there is no detectable effect for women with insurance, and while the effects for women without insurance are of comparable size to those at large schools, the estimates are exceedingly noisy, likely due to the sharply reduced sample size. This issue carries over to the last panel, which again presents results with a full set of time dummies. The point estimates for the difference in treatment between large and small schools are quite close to zero, but the large standard errors—especially for women without health insurance—render them largely uninformative.

Table 3.3b shows that students without health insurance are also more severely affected in the NSFG.⁵⁰ While the estimates for women with continuous health insurance cluster around 0 in both panels and whether we control for demographics or use individual fixed effects, the estimates for uninsured women are large, negative, and statistically

⁵⁰ It is worth noting that the question about insurance is slightly different between the two surveys. In the first NCHA survey, the insurance question is “Do you currently have health insurance?”; the later survey changes the question to “What is your primary source of health insurance?” with “I don’t have health insurance” as one of the response options; in the NSFG the question is “Have you had health insurance continuously for the last 12 months?” While the responses are likely to be highly correlated, they are not completely comparable.

significant. Indeed, the fixed-effects point estimates for both the difference-in-differences (relative to non-college women) and differences (among college women) are identical at -8.6 percentage points, which would be expected under a null treatment effect on non-college women. Although this estimate is more than twice as large as the NCHA estimate, differences in the insurance question on the surveys prevent them from being strictly comparable. Rather, we view it as strong additional evidence for heterogeneous treatment effects by insurance status.

Table 3.3b: NSFG – Pill Use in Month by Insurance Status

	Has Health Insurance	No Health Insurance	Has Health Insurance	No Health Insurance
A: All Women				
Mean DV HI= 0.190	-0.0257	-0.0649	0.0091	-0.0865**
Mean DV NHI= 0.154	[0.0312]	[0.0513]	[0.0249]	[0.0395]
N(obs) = 159,675				
n(women) = 3,695				
B: Students Only				
Mean DV HI = 0.332	0.0058	-0.0896**	0.0263	-0.0863**
Mean DV NHI= 0.266	[0.0311]	[0.0380]	[0.0202]	[0.0349]
N(obs) = 14,637				
n(women) = 576				
Demographic Controls or FE	D	D	FE	FE

Standard errors, clustered at the PSU level, are in brackets. Dependent variable is Pill use in a given month. All regressions include population weights. Each cell is a separate regression that is based on a retrospective contraceptive calendar and restricts the sample to women who were interviewed during or after September 2007. Regressions are run separately for each insurance condition. Panel A presents difference-in-differences estimates between college women and non-college women. Panel B presents differences estimates for college women only. Demographic controls include age, education, race, insurance, and poverty status. Insurance status is determined by whether the respondent had continuous health insurance coverage over the previous 12 months.

We extend the analysis of heterogeneous treatment effects by income in Table 3.4, which presents the results of regression (3.5) with Pill use as the dependent variable and credit card debt as the heterogeneous treatment variable.⁵¹ Credit card debt can be thought of as a

⁵¹ Because the later survey instrument removed questions related to credit card debt, these results omit responses from Fall 2008 to Spring 2009.

proxy for financial constraints; women with large credit card balances are more likely to be both lower income and more credit constrained and price sensitive than their peers without balances. Pill use decreased among women without any credit card debt by 1.2 percentage points but decreased by about 3 percentage points among those with positive debt, with even larger effects found among women with higher credit card balances.⁵²

Table 3.4: NCHA – Pill Use by Credit Card Debt

	No debt	Positive debt	No debt	Positive debt
A: All College Women				
Mean DV ND = 0.391	-0.0123*	-0.0301***	-0.0125**	-0.0209**
Mean DV PD = 0.488	[0.0062]	[0.0087]	[0.0061]	[0.0080]
n = 95,886				
B: by School Size				
Large (5,000+)				
Mean DV ND = 0.393	-0.0129*	-0.0340***	-0.0120*	-0.0318***
Mean DV PD = 0.487	[0.0066]	[0.0088]	[0.0064]	[0.0092]
n = 87,860				
Small (< 5,000)				
Mean DV ND = 0.373	-0.0080	0.0445**	-0.0134	0.0497**
Mean DV PD = 0.497	[0.0074]	[0.0205]	[0.0139]	[0.0193]
n = 8,026				
C: by School Size, time FE				
Treat x Large	-0.0016	-0.0751***	-0.0028	-0.0749**
	[0.0084]	[0.0205]	[0.0017]	[0.0283]
School-specific Time Trend			X	X

Standard errors, clustered at the school level, are in brackets. Dependent variable is Pill use at last sex. Sample only includes responses from the first survey instrument (Spring 2000 to Fall 2008). All specifications include demographic controls and omit instrument indicators. About 69 percent of the sample carried no debt. See Table 3.2a for other notes.

When we stratify by school size, we find relatively little difference for students without debt. Among students with debt, on the other hand, there is a large difference stemming from effects for large schools similar to those found in panel A and strangely positive (and sometimes

⁵² In results not shown in the table, the reduction in pill use is monotonically related to debt: Women with credit card balances between \$1 and \$2,000 reduced their use of the Pill by 2.3 percentage points, and women with over \$2,000 in debt reduced Pill use by 4.6 percentage points.

significant) effects for small schools. These positive results are something of a puzzle, although we note they are based on a relatively small sample.

In additional specifications not shown in a table, we have also checked heterogeneity by age⁵³ (NCHA and NSFG), parental education (NSFG), and family poverty (NSFG). We consistently find patterns of stronger negative effects among the lower socioeconomic groups. Together with the results above, a clear pattern emerges of financially disadvantaged women—who were the most likely to benefit from the subsidies at campus health centers—having strong and statistically significant reductions in their Pill usage after the policy change, while more advantaged women could better absorb the price increase. For women without insurance and women with large amounts of debt, for whom the price increase is more likely to bind, we estimate Pill usage to have decreased between 5 and 10 percent. Although this implies a relatively small price elasticity for the Pill, consistent with the previous literature, it is clear the price elasticity is greater for vulnerable populations.⁵⁴

3.7.2 Other Methods and Sexual Behavior

As explained in the theoretical framework, if college women are using the Pill less, they could also be changing their birth control choice and sexual behavior. Results for other birth control methods as dependent variables are reported in Table 3.5. Each column is a separate contraceptive method, and with the exception of the last, emergency contraception (the “morning-after” pill), they are mutually exclusive. For brevity, results from specifications that

⁵³ Given that the modal age to graduate college is 22, women over 23 are more likely to be graduate students or non-traditional students who are less likely to have parental support and more likely to be financially constrained.

⁵⁴ Caution should be exercised in estimating an actual elasticity, as we do not observe the fraction of college women who received their birth control pills at college health centers before the policy change. We address this issue in the next section.

include school-specific time trends are suppressed (they are very similar).

Table 3.5: NCHA – Other Contraceptive Methods

	<i>Non-Pill Rx</i>	<i>Condom, no Rx</i>	<i>Other non- Rx</i>	<i>Nothing</i>	<i>Emergency contraception</i>
Mean DV	0.0665	0.1588	0.0779	0.0410	0.0934
A: All College Women	-0.0080** [0.0036]	0.0023 [0.0045]	0.0033 [0.0032]	0.0024 [0.0021]	0.0035 [0.0037]
B: by School Size, time FE	-0.0027 [0.0059]	-0.0170*** [0.0061]	-0.0074 [0.0076]	0.0051 [0.0031]	-0.0015 [0.0068]

Notes: Standard errors, clustered at the school level, are in brackets. Each cell is a separate regression. Non-Pill Rx includes implants, injectables, patches, rings, and IUDs. Other non-Rx includes spermicide, fertility awareness, and withdrawal. The first four columns are mutually exclusive and measured at last sex; women who have not had sex are coded as a 0 for each outcome. Emergency contraception (the “morning after” pill) is for use over the last school year (1st instrument) or last 12 months (2nd instrument). All regressions include demographic controls and results from both survey instruments. See Table 3.2a for other notes.

In general, there is little indication of substitution toward other methods. While the interrupted time-series coefficients (panel A) are mildly positive for non-prescription methods, they are not close to statistical significance, and they are not especially robust to a difference-in-differences strategy, as shown in panel B. If anything, the latter strategy suggests affected women *reduced* use of both the condom and other non-prescription methods, while slightly increasing reliance on no method at all. The interpretation of these estimates requires caution, however, as we have chosen to code the outcome variable as 0 for women who have not had sex. This coding particularly affects the estimate for the use of no method: among women who are sexually active (not shown), the point estimate [standard error] is 0.0046 [0.0028] in panel A and 0.0095* [0.0046] in panel B. This suggests that the policy change increased the rate of unprotected sex among women who remained sexually active. Thus, some of the pattern may be driven by a response along the sexual behavior margin, which we investigate directly in

Table 3.6.

Table 3.6: NCHA – Sexual Behavior

	<i>Ever had sex</i>	<i>Had sex in last 30 days</i>	<i>In serious relationship</i>	<i>1+ male partners</i>	<i>2+ male partners</i>
Mean DV	0.689	0.503	0.462	0.664	0.214
A: All College Women	-0.0151** [0.0068]	-0.0088 [0.0093]	-0.0255*** [0.0059]	-0.0231*** [0.0054]	-0.0184*** [0.0045]
B: by School Size, time FE	-0.0327** [0.0125]	0.0124 [0.0151]	0.0137 [0.0161]	-0.0127 [0.0135]	-0.0232** [0.0111]

Notes: Standard errors, clustered at the school level, are in brackets. Each cell is a separate regression. Each outcome is binary. Sex refers to vaginal sex with a male partner. “Serious relationship” includes students who are married, engaged, or in a committed relationship. The number of sexual partners is over the last school year (1st instrument) or last 12 months (2nd instrument). All regressions include demographic controls and results from both survey instruments. See Table 3.2a for other notes.

The first panel shows a statistically significant reduction in the fraction of women who have ever had sex of 1.5 percentage points (2.2 percent)—approximately the same magnitude as the reduction in Pill use.⁵⁵ This reduction does not seem to extend as much to the intensive margin, as the reduction in sex within the last 30 days is roughly half the magnitude, though the standard errors are larger. The rest of the panel shows there were also reductions of around 2 percentage points each in the fraction that were in a serious relationship, had a male partner within the last 12 months, or had two or more male partners in the last 12 months. These results generally carry over to the difference-in-differences strategy in panel B. The notable exception is the share in a serious relationship, the coefficient of which changes from a statistically significant -0.0255 to an insignificant 0.0137. When we estimated this relationship separately by school size (as in panel B of Table 3.2a) we found negative effects for both small

⁵⁵ In a static population, it is not possible to have a reduction in the fraction of women who have ever had sex. The population of college students is not static, however, as new freshmen arrive each year.

and large schools, with a greater magnitude at smaller schools. It is hard to say whether the effect on relationship status is simply spurious or whether an additional, unobserved factor affected smaller schools in the treatment period. Even remaining agnostic about this outcome, there is reasonable evidence that college women responded to the price change in prescription contraception by reducing sex altogether. Recall that the discrete choice framework highlighted how a rise in birth control prices could affect both the equilibrium frequency of sex and whether an individual stayed (entered) in a coupled relationship. In practice, it is not clear whether behavior more closely resembles what is shown in Figure 3.2c or what is in Figure 3.2b, but s clearly falls.

Overall, these results show that college women did reduce their use of the Pill in response to the price increase, and that the reductions were two to three times larger among lower income or credit-constrained women and women without health insurance. Additionally, we find that the largest behavioral response was not switching to other forms of contraception so much as a reduction in overall sexual behavior.

3.8 Bounding the Price Elasticity

As we have noted, our estimates of the effect of the price increase in prescription contraceptives at college health centers are intention-to-treat effects, since not all women assigned to the treatment regime were affected by the policy shift. An important limitation of our data is that we do not observe whether college women were filling their birth control prescriptions at university health centers (and thus would be subject to the policy change) or at a retail pharmacy, Planned Parenthood clinic, or elsewhere. In fact, to our knowledge, *no* large-

scale survey has asked college women who use prescription birth control where they obtained it.⁵⁶ From a policy perspective, and for comparisons with experimental studies in developing countries (e.g., Schwartz et al. 1989; Ciszewski and Harvey 1995), such information is essential. For example, our intent-to-treat estimates, by themselves, do not indicate whether a large share of college women was only slightly affected or whether a relatively small share was significantly affected. That is, without knowing the denominator of the fraction of college women using birth control who obtain it from the university health center, we cannot speak to the treatment effect on the treated or calculate price elasticities of demand.

In order to fill this gap, we fielded a survey of 860 female students at a large, research university in the Midwest.⁵⁷ The approximately 30-question survey asked many of the demographic questions that appear in either the NCHA or NSFG (to allow for conditional comparisons), but focused on contraceptive methods. For prescription-based methods, women were asked both where prescriptions were obtained *and* where they were filled. As such, we can begin to answer the questions posed in the previous paragraph, albeit with two, nontrivial caveats. First, our survey was fielded at a single university that is not representative of all colleges and universities in the country: in particular, the students are almost exclusively of traditional age and disproportionately come from affluent families. Second, while it would have been preferred to survey students before the 2007 policy change, our survey was fielded in

⁵⁶ The more recent waves of the NSFG, including the one used in this study, do ask women where they *obtained* the prescription but not where it was *filled*. The MEPS data do have some information on where prescriptions are filled, but the “clinic” option combines walk-in/community clinics, Planned Parenthood offices, hospitals, HMOs, and university clinics together.

⁵⁷ The details of how the survey was fielded, sample sizes, and response rates can be found in the appendix. Appendix Table 3.9 describes characteristics of the sample.

May of 2011, nearly four years afterward.⁵⁸ Thus, if the policy change affected where women acquired their birth control, the survey responses will be biased as estimates of the pre-DRA period. We discuss below how both of these issues affect our estimation.

The first four rows of Table 3.7 show the fraction of female students who have used prescription birth control while at college. Approximately 56 percent of our sample had, and 74 percent had among the sexually active.⁵⁹ Columns 3 through 8 stratify the sample by age, financial aid status, and whether the student has insurance that covers prescriptions. Students age 25 or older are more likely to have used prescription contraceptives, but this seems to be driven by a greater propensity to be sexually active. Women who lack prescription drug insurance coverage, on the other hand, are notably less likely to have used prescription contraceptives, even among the sexually active. Indeed, the bottom of the table shows that these women are by far the most likely to cite price as being an important determinant of where to fill a prescription for birth control.

The middle panels of the table show where college women obtain their prescription for contraception and where they fill that prescription. Nearly half of women get their prescription from an off-campus primary care physician, while about 40 percent do so from the university clinic. Surprisingly, few students in our sample go to off-campus clinics such as Planned Parenthood, but this may be a feature of the specific nature of our sample. Indeed, women who lack prescription drug coverage are twice as likely to use an off-campus clinic as women who have coverage, and they are also more likely to use the university clinic.

⁵⁸ We thought about asking retrospective questions on birth control, but the timing is such that very few women would have been in college before the policy change.

⁵⁹ As in the NCHA data, some 90 percent of prescription birth control is in the form of the Pill.

Table 3.7: Sources for Rx Birth Control Among College Students

	All Students	Age 24 or younger	Age 25 or older	On Finan. Aid	Not on Finan. Aid	Insurance covers Rx	Insurance doesn't cover Rx
Use Rx BC	0.559	0.525	0.687	0.581	0.525	0.604	0.417
Use Rx BC sexually active	0.743	0.744	0.741	0.743	0.739	0.781	0.611
Use Rx BC in last 30 days	0.385	0.381	0.400	0.390	0.385	0.423	0.266
Use Rx BC in last 30 days sex in last 30 days	0.634	0.679	0.518	0.616	0.663	0.670	0.502
Obtained most recent Rx from:							
off-campus PC physician	0.488	0.536	0.349	0.426	0.591	0.521	0.337
off-campus clinic	0.056	0.055	0.058	0.062	0.044	0.046	0.103
university clinic	0.386	0.352	0.483	0.444	0.289	0.363	0.490
Elsewhere	0.071	0.057	0.110	0.069	0.076	0.071	0.071
Filled most recent Rx at:							
stand-alone drug store	0.426	0.419	0.447	0.417	0.450	0.446	0.333
pharmacy in larger store	0.160	0.177	0.107	0.145	0.193	0.140	0.250
off-campus clinic	0.033	0.035	0.029	0.036	0.031	0.026	0.068
university clinic	0.202	0.191	0.235	0.221	0.167	0.196	0.229
mail/online pharmacy	0.069	0.086	0.017	0.055	0.076	0.074	0.047
university physician	0.024	0.002	0.088	0.033	0.007	0.026	0.012
off-campus PC physician	0.042	0.050	0.021	0.062	0.009	0.049	0.009
Elsewhere	0.044	0.040	0.057	0.032	0.068	0.042	0.052
Factor is important or very important in deciding where to fill Rx:							
price/cost	0.723	0.702	0.783	0.763	0.637	0.676	0.935
convenience/location	0.827	0.813	0.867	0.832	0.820	0.843	0.755
availability/choices	0.258	0.247	0.292	0.269	0.222	0.268	0.210

Notes: The numbers shown represent the fraction of sample women from the university birth control survey answering each option. The sample size is 860 for rows 1 and 3 of the table, 622 for row 2 (which conditions on having had vaginal intercourse while at college), and 443 for row 4 (which conditions on having had vaginal intercourse in the 30 days prior to survey). The panels for obtaining and filling Rx, and the factors that are important in determining where to fill Rx, are conditional on having used Rx BC in college (row 1). The estimates reflect post-hoc weights to make the sample representative of the university on the basis of age, race, and class standing. Source: Authors' survey; see Survey Appendix.

Students on financial aid and those above age 24, who are less likely to be dependents of their parents, are also more likely to use the university clinic, suggesting that relative income plays an important role in obtaining prescription contraception. As for where women fill the prescription, the most popular choice by far is a stand-alone drug store or pharmacy, but the university clinic and pharmacy is second, used by 20 percent of women. Again, socioeconomic status matters, with older students, those on financial aid, and those without drug coverage more likely to use the university pharmacy, although the difference is only a few percentage points and is generally not statistically significant.

If we were to assume that the 20 percent of sampled women who obtained their prescription birth control at the university pharmacy was a reliable estimate of the share of all college women nationally who did so before the 2007 policy change, *and* that these were the only group of women affected by the policy change, then we could calculate an average treatment effect on the treated (ATT) by dividing our baseline estimate from Table 3.2a, panel A by 0.202 (Angrist and Imbens 1995). While the latter assumption of no spillovers from the policy change seems plausible, the former, as we have suggested, is problematic. Students at the university in our survey are financially much better off than the typical college student nationally, and the socioeconomic gradient in where students obtain their prescription contraception suggests that the 20 percent figure is likely too low for a national estimate. To gauge how much this compositional effect matters, we reweighted the data on the basis of race, age, class standing, financial aid receipt, and the education of parents to conform to the national average of students attending a four-year college or university in the 2008 National Postsecondary Student Aid Study. This changed the fraction using the university pharmacy from

0.202 to 0.244.

It is also unclear how fielding the survey in the spring of 2011 yields estimates different than what would have obtained had the survey taken place in 2006. Standard choice theory (and the relative importance of price and convenience shown in Table 3.8) predicts that the higher prices in the post-change regime would have induced some women to switch away from the university pharmacy and toward the next best substitute—possibly pharmacies in “big box” stores, which most closely resemble the cost and convenience attributes of the campus pharmacy. Thus, it seems reasonable that this bias, too, is downward, although the magnitude is hard to gauge.

For these reasons, we believe the 0.202 estimate is a lower bound for the fraction of all college students who obtained their prescription birth control from the campus pharmacy prior to the DRA. To get a plausible upper bound, we apply the adjustments for composition bias above and assume, as an extreme case, that the policy change caused fully half of students who had been filling their prescription at the campus pharmacy to switch providers. This yields an estimate of 0.488 ($0.244/0.5$). Using our (conservative) benchmark estimate from column 2, panel C of Table 3.2a, this means that the ATT is effectively bounded between -3.1 ($-0.015 / 0.488$) and -7.4 ($-0.015 / 0.202$) percentage points. Relative to the mean pill use rate of 0.409, the ATT is a 7 to 18 percent reduction. Furthermore, we know that the average price paid by college students for prescription birth control (relative to that paid by non-students) increased by \$3.97 per month between 2007 and 2008 (see Figure 3.2).⁶⁰

⁶⁰ The increase is nearly identical if 2006 is used as the base year instead.

Table 3.8: Importance of Determinants of Sources for Rx Birth Control, by Source, Among College Students

	Price is important/very important	Convenience/Location is important/very important	Availability/Choice is important/very important
Filled most recent Rx at:			
stand-alone drug store	0.632	0.814	0.275
pharmacy in larger store	0.849	0.810	0.238
off-campus clinic	1.000	0.770	0.269
university clinic	0.815	0.920	0.211
mail/online pharmacy	0.746	0.806	0.208
university physician	0.788	0.625	0.480
off-campus PC physician	0.540	0.769	0.339
Elsewhere	0.580	0.884	0.281

Notes: The numbers shown represent the fraction of sample women from the university birth control survey who obtained their prescription birth control from the source indicated that considered the specified factors as important or very important in determining where they filled their prescription. The estimates reflect post-hoc weights to make the sample representative of the university on the basis of age, race, and class standing. Source: Authors' survey; see Survey Appendix.

If this price increase was driven entirely by campus pharmacies, then the effective price increase among the treated was between \$8.14 and \$19.65 per month ($\$3.97 / 0.202$), which is roughly in line with prices tripling from a base of \$7 to \$8 (Rooney 2007). A back-of-the-envelope calculation shows that the price elasticity of demand is between -0.09 ($-18/200$) and -0.035 ($-7/200$) under a conservative 200 percent effective price increase. These elasticities would be halved if the price increase were 400 percent (quintupling).

Although crude, these numbers represent the first estimates in the literature of the price elasticity of demand for prescription birth control among college women in the U.S. With little doubt, this demand is price inelastic, with even the high end estimate still below 0.1 in absolute value for college women as a whole. Given the strength of the assumptions underlying these ATT and elasticity estimates, we are reluctant to quantify these statistics for the subgroups of college women elsewhere in the analysis; however, because location choice appears less sensitive to price or income than pill use itself, the ATTs and elasticities for less advantaged groups are almost certainly larger in magnitude than they are for all college women, although they are likely to still be quite inelastic.

3.9 Conclusion

In this paper, we examine the effects of a large exogenous shock to the price of the birth control Pill on college campuses caused by the Deficit Reduction Act of 2005. Using two different data sources, the NCHA and the 2006-2008 NSFG, we find that the three-to-ten-fold increase in the price of the Pill reduced the use of oral contraception by about 1.7 to 2.5 percentage points (4 to 6 percent) among college women. These findings are consistent with

previous literature that documents small price elasticities for contraception in other contexts. We also find evidence that the reduction in the use of the Pill was significantly stronger for women without health insurance, women with credit card debt, and older women—groups for whom the price increase was most likely to bind. Although there is some suggestive evidence of an increase in unprotected sex among women who remain sexually active, one of our more robust findings is that the price increase led to a reduction in sexual behavior overall, with affected women reporting a lower likelihood of having had sex and fewer sexual partners. The analysis of the NSFG data, including results not reported in the tables in this paper, returns broadly consistent estimates, increasing our confidence in the NCHA results.

Our findings suggest that the enactment of the Deficit Reduction Act and the consequent increase in the price of the Pill on college campuses had a modest but economically meaningful effect on the contraceptive choices of college women, primarily by reducing Pill usage in populations that are likely to be financially constrained. Because we do not observe where women in our sample obtain their birth control—and thus which women are directly affected by the price increase—our methodology estimates intention-to-treat effects on the entire population, and our estimates are lower bounds on the average treatment effect on the treated. In order to be more precise about the relationship between these two parameters, we fielded a supplementary survey of college students that specifically asked where they filled their birth control prescriptions. While far from ideal, the survey allows us to construct an independent measure of the denominator in the Wald estimator that relates our ATE to the ATT. When we make the standard assumptions in the treatment effects literature (Angrist and Imbens 1995), we estimate that the ATT is approximately four times the ATE, or that women

who had been obtaining their birth control at campus pharmacies prior to the price change reduced their Pill use by roughly 16 percent. It is likely that many more still used the Pill but switched to a more expensive provider, although we cannot investigate this directly due to data limitations.⁶¹ Nonetheless, incorporating this information with mean pill use and data on the effective price change, we can bound the price elasticity of demand for Pill use among this population of college women as between -0.09 and -0.035, the first such estimates in the literature in the American context.

With provisions of the Affordable Care Act (ACA) now in effect that require nearly all health insurance plans to cover contraceptives, including the Pill, without deductible or co-pay, *and* additional provisions that are likely to increase the percentage of young Americans with health insurance (the “mandate”), it is of direct and immediate policy interest how this effective decrease in the price of prescription contraception will affect use and related sexual behavior. To the extent that our estimates are relevant to the population affected by the ACA, we would expect a relatively modest increase in prescription contraceptive use, on the order of 10 percent, for a 90 percent price reduction.⁶²

⁶¹ In interviews with campus pharmacy directors, we were told that some students switched their brand-name prescriptions for cheaper generics at the campus pharmacy, while others stopped using the campus pharmacy and went to drug stores or big box stores with pharmacies instead.

⁶² Of course, already-contracepting women have their utility increased due to an income effect.

3.10 References

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3.11 Survey Appendix

With the cooperation of the university health services (of which the campus pharmacy is part) and the registrar's office, and in consultation with the university's institutional review board (IRB), we designed a 31-question survey intended for students of the university. (The survey instrument follows this appendix.) The survey was hosted electronically by Qualtrics, and students completed it via web browser.

Five thousand students enrolled at the university in May 2011 and who were at least age 18 were randomly drawn by the registrar's office and invited by email to participate in the survey. As an incentive, students completing the survey were entered into a drawing to win two \$50 gift certificates from Amazon.com. The initial email was sent on May 3, 2011, and follow-up emails were sent on May 11 and May 19 reminding those who had not yet participated about the survey. The survey was live throughout the month of May.

Of the 5,000 students invited, 1,439 (29 percent) began the survey and 1,329 (27 percent) completed it. Of these 1,329 completers, nearly all provided key demographic information on gender, race, and class standing; only 17 did not. Our effective sample thus consists of 1,312 students, among whom 860 (66 percent) are women and 452 (34 percent) are men. As the sample frame was a simple random sample and was not stratified, response rates were not proportional to the enrolled universe of students. Using data from the university and the IPEDS database, we constructed two types of sampling weights: one based on sex-race-class cells and one based on (marginal) age categories.⁶³ The weights were created such that the product of the in-sample cell proportion and the weight equaled the universe proportion. A final weight was created by taking the product of the sex-race-class weight and the age-group weight. As our focus is on women, we present in Appendix Table 3.9 selected summary statistics for the women sample, both weighted and unweighted.

⁶³ Data limitations prevented weights based on sex-race-class-age group cells.

Appendix Table 3.9: Respondent Demographics

	<u>Unweighted Percentage</u>	<u>Weighted Percentage</u>
Age: 18 or 19	28.8	43.4
Age: 20 or 21	31.1	24.5
Age: 22 to 24	17.6	11.3
Age: 25 to 29	17.1	12.6
Age: 30+	5.4	8.2
Race: White	70.4	69.2
Race: Black	4.3	5.3
Race: Hispanic	2.0	5.7
Race: Asian	16.1	12.5
Race: Other/Multi	7.3	7.3
Class: Freshman	5.5	17.2
Class: Sophomore	11.1	16.3
Class: Junior	20.6	17.3
Class: Senior	24.0	18.9
Class: Other UG	2.3	0.3
Class: Graduate/Prof	36.6	30.1
Mom's educ: < HS	2.7	3.1
Mom's educ: HS	11.5	11.8
Mom's educ: Some coll	18.5	18.0
Mom's educ: BA	36.1	34.4
Mom's educ: > BA	31.1	32.8
Dad's educ: < HS	3.2	3.0
Dad's educ: HS	11.3	12.9
Dad's educ: Some coll	14.7	13.1
Dad's educ: BA	28.2	27.6
Dad's educ: > BA	42.6	43.4
Financial Aid: Yes	63.8	62.1
Financial Aid: No	35.3	37.2
Credit card debt: No card	32.3	37.4
Credit card debt: Card, no balance	42.6	39.1
Credit card debt: < \$500	11.0	10.3
Credit card debt: \$500 - \$999	6.0	4.8
Credit card debt: \$1000 - \$1999	3.7	4.1
Credit card debt: \$2000+	4.4	4.4

N

860

3.11.1 Survey Instrument

1. Are you currently a
 - a. First-year undergraduate student
 - b. Sophomore
 - c. Junior
 - d. Senior
 - e. Other undergraduate student
 - f. Graduate or professional student
2. In what year were you born?
3. Are you
 - a. Male
 - b. Female
 - c. Transgender
4. What is your race/ethnicity? (check all that apply)
 - a. White
 - b. Black
 - c. Hispanic or Latino
 - d. Asian or Pacific Islander
 - e. American Indian or Alaskan Native
 - f. Other
5. What is your mother's highest level of education?
 - a. less than high school
 - b. some high school
 - c. high school diploma
 - d. some college
 - e. bachelor's degree
 - f. master's/professional degree
 - g. doctoral degree
6. What is your father's highest level of education?
 - a. less than high school
 - b. some high school
 - c. high school diploma
 - d. some college
 - e. bachelor's degree
 - f. master's/professional degree
 - g. doctoral degree
7. What is your major, intended major, or graduate program?
 - a. Humanities
 - b. Social science
 - c. Engineering
 - d. Science
 - e. Arts

- f. Other, specify:
8. What is your current relationship status?
- a. Single (not currently dating)
 - b. Dating or hooking up, but not seriously
 - c. Steady, serious partner
 - d. Cohabiting with partner and/or engaged
 - e. Married
 - f. Separated, divorced, or widowed
9. Do you currently have health insurance?
- a. Yes
 - b. No
 - c. I don't know
10. Does your health insurance cover prescription medications? [if Yes in 9.]
- a. Yes
 - b. No
 - c. I don't know
11. Are you currently on your parents' or guardians' health insurance?
- a. Yes
 - b. No
 - c. I don't know
12. Have you ever been to University Health Services (UHS) for medical services or advice?
- a. Yes
 - b. No
13. Have you ever filled a prescription at UHS?
- a. Yes
 - b. No
14. How many times have you visited a health care professional in the last 12 months?
15. Of those visits, how many were at UHS (University Health Services)? [if yes in 12.]
16. Have you ever had vaginal sex?
- a. Yes
 - b. No
17. Have you had vaginal sex within the last 30 days? [if yes in 16.]
- a. Yes
 - b. No
18. While at the University of Michigan, have you or your partner(s) **ever** used any of the following kinds of contraception? (check all that apply)
- a. None; not sexually active
 - b. None, sexually active
 - c. Condoms
 - d. Birth control pill
 - e. NuvaRing (vaginal ring)
 - f. Patch

- g. DepoProvera (a shot)
 - h. IUD (intra-uterine device)
 - i. Norplant/Implanon (implant)
 - j. Diaphragm
 - k. Fertility Awareness (rhythm, calendar, or safe period)
 - l. Withdrawal (pulling out)
 - m. Sterilization
 - n. I don't know
19. **Within the last 30 days**, have you or your partner(s) used any of the following kinds of contraception? (check all that apply)
- a. None; not sexually active
 - b. None, sexually active
 - c. Condoms
 - d. Birth control pill
 - e. NuvaRing (vaginal ring)
 - f. Patch
 - g. DepoProvera (a shot)
 - h. IUD (intra-uterine device)
 - i. Norplant/Implanon (implant)
 - j. Diaphragm
 - k. Fertility Awareness (rhythm, calendar, or safe period)
 - l. Withdrawal (pulling out)
 - m. Sterilization
 - n. I don't know
20. Where did you get the most recent prescription for your birth control? [if d, e, f, g, h, i, or j in 18. and b in 3.]
- a. Your primary care physician off-campus
 - b. A non-campus clinic (Planned Parenthood, community clinics, etc.)
 - c. UHS
 - d. Somewhere else
specify:
21. Where did you *fill* the most recent prescription for your birth control? [if d, e, f, g, h, i, or j in 18. and b in 3.]
- a. Stand-alone drug store (Walgreens, CVS, etc.)
 - b. Pharmacy in a larger store (Wal-Mart, Costco, Meijer, etc.)
 - c. A non-campus clinic (Planned Parenthood, community clinics, etc.)
 - d. UHS pharmacy
 - e. Mail-order or online company
 - f. UHS clinician [only if g, h, i, or j in 18. and b in 3]
 - g. Your primary care physician off-campus [only if g, h, i, or j in 18. and b in 3]

22. On a scale of 1 to 5, where 5 is most important and 1 is least important, how much do each of the following factors matter in determining where you fill your birth control prescription?
- Price
 - Location/convenience
 - Wide availability of different brands
 - Other (specify)
23. Certain medical services, such as an HIV or other sexually-transmitted infection (STI) test, a pregnancy test, abortion, or prescription birth control, are often covered by health insurance. Have you ever declined one of these services or paid for it entirely out-of-pocket due to privacy concerns?
- Yes
 - No
 - Not sure
24. Have you ever declined or paid entirely out-of-pocket for an HIV or other STI test, a pregnancy test, abortion, or prescription birth control because you were worried your parents would find out if you billed insurance?
- Yes
 - No
 - Not sure
25. Have you ever received any kind of contraception (condoms, birth control prescriptions, etc.) from UHS?
- Yes
 - No
 - Not sure
26. Were you aware that UHS offers the following free and confidential services to students?
- Yes No
- Pregnancy tests
 - Contraception-related visits
 - STI tests (gonorrhea, chlamydia, etc)
 - HIV tests
27. Now that you know UHS offers these services freely and confidentially to students, how likely are you to use UHS if you were in need of such services?
- Very likely Somewhat likely Not very likely Very unlikely Don't Know
- Pregnancy tests
 - Birth control prescriptions
 - STI tests (gonorrhea, chlamydia, etc)
 - HIV tests
28. If you have a credit card, how much total credit card debt did you carry last month? That is, what was the total unpaid balance on all of your cards that you are responsible for paying?
- Don't have own credit card
 - Have own credit card, but don't carry a balance
 - Less than \$500
 - Between \$500 and \$1000
 - Between \$1000 and \$2000

- f. More than \$2000
- 29. Do you receive financial aid (grants or loans) at the University of Michigan?
 - a. Yes
 - b. No
- 30. Roughly how much of your tuition and fees does financial aid cover? [if a in 29]
 - a. All
 - b. Most (over 50%)
 - c. Some (less than 50%)
 - d. None
- 31. Roughly how much of your living expenses does financial aid cover? [if a in 29]
 - a. All
 - b. Most (over 50%)
 - c. Some (less than 50%)
 - d. None

CHAPTER IV

Achieving the Nuclear Family: The Impact of the Birth Control Pill on Spacing and Stopping in the 1960s

4.1 Introduction

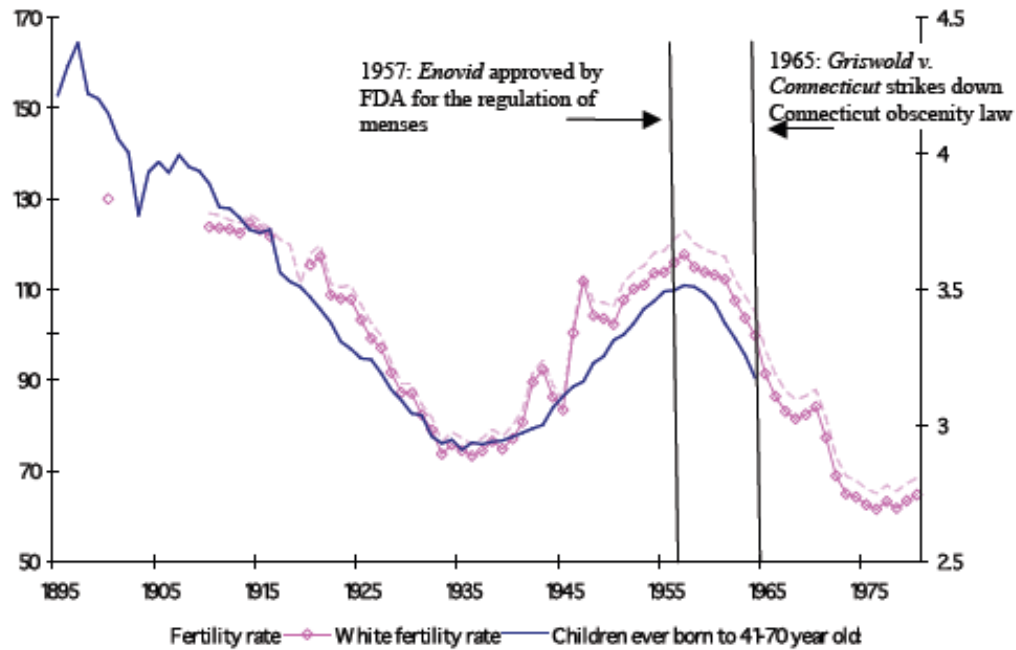
In their “Millenium Issue,” *The Economist* declared the birth control pill to be the defining invention of the 20th century. Though other contraceptive devices existed previously, the Pill was unique in its efficacy and in the autonomy it afforded women in controlling their fertility, and women adopted it quickly. Ten years after its approval by the FDA it was used by more than 25 percent of American women of reproductive age (Westoff and Ryder 1977) and by 1987 this number exceeded 80 percent (Dawson 1990). Despite its wide usage in the U.S. and elsewhere, the true impacts of the Pill on the way women and couples controlled their fertility remain poorly understood.

As use of the birth control pill has become commonplace in the developed world and issues of population sustainability are becoming more important in the developing world, understanding the true impact of access to the Pill is increasingly important for policy-makers. Even today in the United States, easy, reliable access to the Pill and other forms of contraception (particularly emergency contraception) is no longer guaranteed for women in more conservative and rural communities, and despite the passage of the Affordable Care Act, recent cuts to state and federal family planning programs have dramatically increased the cost

of obtaining the Pill for low-income women in states without expanded Medicaid access. In order to fully assess the impact of expanding or restricting access to the Pill (particularly through changing the costs of obtaining the Pill), we first need a rigorous understanding of the causal impact of access to the Pill.

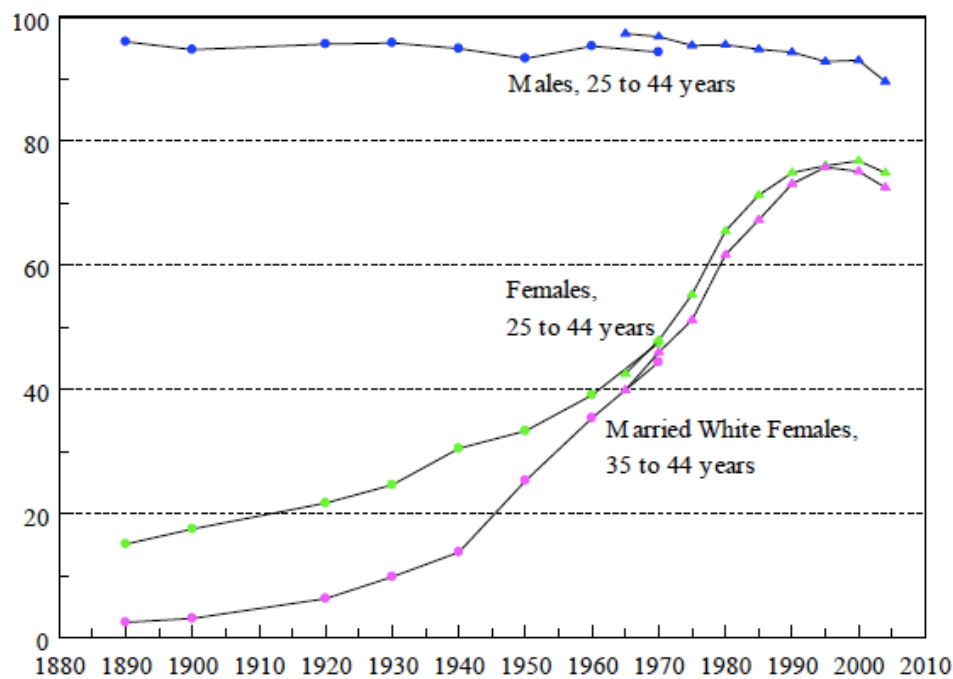
Although the Pill has been lauded as one of the great technological advances of the twentieth century, significant scholarly debate surrounds the role of the Pill in the dramatic demographic changes of the 1960s. Demographers have tended to support the view that the improvement in women's ability to regulate their births brought about by the Pill was a key component of the dramatic decline in U.S fertility rates in the 1960s. Economists, on the other hand, have tended to emphasize changes in the demand for children, which occurred independently of advancements in contraceptive technology. As Becker said in his *Treatise on the Family*, "the 'contraceptive revolution' ... ushered in by the Pill has probably not been a major cause of the sharp drop in fertility in recent decades" (Becker 1991). The difficulty in quantifying the Pill's impact stems from the coincidence of its release with the myriad social and demographic changes of the late 1950s and early 60s—the baby boom peaked in 1957, the women's movement gained traction, and increasing numbers of women joined the labor force. In the twenty years from 1955 to 1975, the U.S. fertility rate fell by more than 50 percent (Figure 4.1). Simultaneously, the percentage of married women in the labor force increased from 30 to more than 50 percent (Figure 4.2, Goldin 2006). The multiple social and economic changes occurring simultaneously with the introduction of the Pill make standard empirical strategies that rely on intertemporal comparisons more challenging.

Figure 4.1: U.S. Fertility Rates and Children Ever Born



Notes: This figure plots the mean children ever born to 41-70 year old women (excluding women who had no children), the period fertility rate, and the white period fertility rate. Source: Bailey (2010).

Figure 4.2: Male and Female Labor Force Participation Rates by Age and Marital Status



The figure plots the percentage of men and women in the labor force, using data from the 1970-1980 censuses and the 1965-2004 March CPS. Source: Goldin (2006).

Recent empirical research has advanced our understanding of the impact of the Pill by developing novel quasi-experimental strategies to isolate its role. Pioneering work by Goldin and Katz (2002) used cross-state, within-birth-cohort variation in the age at which women could legally obtain the Pill to estimate its impact on age at first marriage and career choices of college graduates. The impetus for these legal changes was a desire in some states to confer more rights on 18 year old men, who were eligible to be drafted in the Vietnam War but could not vote. One important, *indirect* effect of these legal statutory changes was to lower the age of majority, allowing unmarried women under the age of 21 to obtain medical care, including oral contraception, without parental consent in some states. Using variation in these laws, Goldin and Katz (2002) find early legal access to the Pill reduced the fraction of women married by age 23 and increased the share of women entering professional schools. Bailey (2006) used a variant of their approach to examine changes in U.S. women's life cycle labor-force participation, finding that access to the Pill before age 21 reduced the likelihood of becoming a mother before age 22 and increased the extent of women's labor-force participation from 26 to 30 years old by 8 percent. But because these natural experiments only manipulated access to the Pill among unmarried women under the age of 21, this empirical strategy does not quantify the importance of the Pill for the population of married women or women ages 21 or older when the Pill was introduced—that is, more than 90 percent of women giving birth during the 1960s.

To investigate this question directly, Bailey (2010) developed an alternative empirical approach to isolate the Pill's impact on the period fertility rates of married women during the 1960s. She exploits idiosyncrasies in the language of state anti-obscenity statutes, better known

as “Comstock laws,” many of which banned the sales of contraceptives, and the timing of two events: the introduction of *Enovid* in 1957, which was later approved by the FDA as the first oral contraceptive in 1960, and *Griswold v. Connecticut*, the 1965 U.S. Supreme Court case that struck down Connecticut’s ban on the use of contraceptives. Bailey (2010) estimates that roughly 50 percent of the decline in the U.S. period fertility rates in the 1960s can be attributed to the introduction of the Pill.

Although Bailey (2010) is able to address the question of the impact of access to the Pill for married women, because the identifying variation is at the state level, the analysis can only include region-by-year fixed effects, which leaves open potential state-by-year threats to identification. Many of the time-varying changes of concern such as abortion legalization, family planning funding, and child welfare benefits vary at the state level. To address these potential threats to identification in Bailey (2010), this paper exploits within-state variation in the distance to the closest border of a state that does not have a restrictive sales ban to capture differences in the cost of the Pill. Specifically, we compare outcomes for women who live within 50 miles from a non-restrictive border and women living more than 50 miles from a non-restrictive border in the same state. This distance serves as a proxy for the time and travel costs associated with obtaining the Pill for women in restrictive sales ban states.

Distance to early legalizing states has been used in the abortion literature as a measure of the cost of access and the literature has shown that women crossing state borders to receive abortions attenuate the estimated results (Levine et al. 1999, Ananat et al. 2009). It is likely that if women crossed state borders to obtain abortions, they would also cross borders to obtain the Pill. As a result, women living further from non-restrictive states should have faced higher costs

to obtain the Pill, whereas women living closer to non-restrictive states (or those directly on the border) should have faced smaller or negligible costs. Additionally, this empirical strategy uses a smaller unit of aggregation, county group, providing variation that is substantially less likely to be correlated with other state-level policy changes or unobservables, narrowing the scope for omitted variables bias in assessing the causal impacts of access to the Pill, while also allowing the inclusion of state-by-year fixed effects.

An important and unanswered question in Bailey (2010) and the related literature is whether the Pill also contributed to the reduction in completed fertility in the post-1960 period. A large component of the fertility decline may have been temporary, as married women in their early twenties—those adopting the Pill most rapidly—used it to delay first births and space their subsequent births. It is also possible that the Pill led to permanent declines in fertility, as delays in first births and greater spacing led women to have fewer children over their lifetimes. This paper extends the analysis in Bailey (2010) and aims to identify those effects in both the short- and long-term using the entire population of women in the 1970 and 1980 censuses.

Our central results suggest that access to oral contraception did not change completed fertility. We find slight suggestive evidence that women with higher costs of access may have had more children prior to legalization and fewer children afterwards, leaving total completed fertility unchanged.

Section 4.2 introduces the legal variation and explains the distance measures that we use, Section 4.3 presents a model of fertility under different contraceptive costs and its implications for our study, Section 4.4 describes the data, empirical strategy, and results, and Section 4.5 concludes.

4.2 Historical Background

In 1873, the U.S. Congress passed the “Comstock Act” outlawing the interstate mailing, shipping or importation of articles, drugs, and printed materials used “for the prevention of conception.” By 1900, all but one of the 48 states had enacted their own anti-obscenity statutes to regulate trade in “obscene” or “immoral” information within their borders. These laws were relatively unimportant prior to 1957, because pre-Pill contraceptive methods were often distributed illicitly and could even be mail-ordered from black market providers (Garrow 1994, Tone 2000). But the Pill was distributed by physicians and pharmacists who faced greater penalties for skirting the law, up to and including the loss of their licenses, meaning that small differences in the language of these laws had important implications for access to the Pill in the 1960s. Some states explicitly defined “contraception” as an obscenity, which effectively banned it. Other states additionally banned the sales of contraceptive supplies, though some of those states had exceptions for licensed physicians. Bailey (2010) shows that these minor differences in wording provide natural randomization of the cost of obtaining the Pill, and that they do, in fact, lead to significant differences in behavior.

Table 4.1 groups these anti-obscenity statutes used in this paper into four broad categories:

1. General obscenity statutes banned the dissemination and sale of obscene information, as well as the sale of indecent or immoral “articles or instruments,” but without any explicit mention of contraception as obscene. These states are classified as non-restrictive or permissive.
2. Advertising or information bans in 31 states banned the distribution of information about or advertisement of instruments or medicines intended to prevent of conception. Importantly, these laws did not ban the sale of contraceptives themselves. In states with only this type of

law, if a patient requested a contraceptive prescription, physicians or pharmacists could provide and fill the prescription without violating state law.

3. Sales bans in 24 states extended the advertising bans to explicitly prohibit the sale of anything intended to prevent conception, including articles, instruments, medicines, or secret nostrums. In states with these laws, physicians and pharmacists were banned from fitting diaphragms or filling birth control prescriptions, unless the law contained a physician or pharmacist exception.
4. Physician or pharmacist exceptions in 7 states contained language that provided blanket exceptions for physicians and sometimes pharmacists from advertising and sales bans. Several other states have exceptions for “legitimate business,” which could be interpreted as including physicians and pharmacists. Because the language of legitimate business exceptions does not explicitly include medical professionals, we assume these states did not have physician exceptions.

For the purposes of this paper, we group states into two categories: restrictive and non-restrictive or permissive states. Restrictive states are defined as those states that had both an advertising ban and a sales ban without a physician exception.⁶⁴ It was effectively impossible for a woman in these states to legally obtain the Pill without travelling to another state. Permissive states are defined as states that had no restrictions, only advertising bans, or advertising and sales bans with a physician exception. In these states it was possible for a woman to get a prescription from her doctor and have it filled by a pharmacist without breaking the law. These state-level differences form the basis for our analysis.

⁶⁴ By this definition, there are seventeen restrictive states: Arizona, California, Colorado, Connecticut, Delaware, Illinois, Indiana, Iowa, Kansas, Massachusetts, Mississippi, Missouri, Nebraska, Nevada, New Jersey, Ohio, and Wyoming.

Table 4.1: Comstock Laws Related to Contraception in the Continental U.S. circa 1960

	Advertising bans	Sales bans	Physician exceptions
Alabama			
Arizona	X	X	
Arkansas	X	X	X
California	X	X	
Colorado	X	X	
Connecticut	X	X	
Delaware	X	X	
Florida			
Georgia			
Idaho	X	X	X
Illinois	X	X	
Indiana	X	X	
Iowa	X	X	
Kansas	X	X	
Kentucky			
Lousiana	X		
Maine	X		
Maryland			
Massachusetts	X	X	
Michigan	X		
Minnesota	X	X	X
Mississippi	X	X	
Missouri	X	X	
Montana	X	X	X
Nebraska	X	X	
Nevada	X	X	
New Hampshire			
New Jersey	X	X	
New Mexico			
New York	X	X	X
North Carolina			
North Dakota			
Ohio	X	X	
Oklahoma			
Oregon	X	X	X
Pennsylvania	X		

Rhode Island			
South Carolina			
South Dakota	X		
Tennessee			
Texas			
Utah			
Vermont			
Virginia			
Washington	X		
West Virginia			
Wisconsin	X	X	X
Wyoming	X	X	

Restrictive states with advertising and sales bans but no physician exemption are shaded. Source: Bailey (2010)

The variation that we exploit in this paper only existed for a limited time—between the introduction of the birth control pill in 1957 and the subsequent elimination of sales bans around 1965. *Enovid*, the first oral contraceptive, was first approved in 1957 for the regulation of menses. Though the FDA did not approve *Enovid* for the purpose of preventing pregnancy until 1960, many women were aware of its contraceptive properties well in advance of the FDA’s decision. As a result, we use 1957 as the date of introduction of the birth control pill for this analysis. In 1965, the Supreme Court struck down Connecticut’s ban on the sale and use of contraceptives in *Griswold v. Connecticut*, prompting states to remove bans on sales of the birth control pill nationwide.

We might worry that the sales bans, while on the books, were not vigorously enforced and, in actuality, did not restrict women’s access to the birth control pill. Bailey (2010) addresses this concern and finds statistically significant differential use of the birth control pill between women in restrictive and non-restrictive states. “Women in states with sales bans were 25 to 30 percent less likely to have ever used oral contraception before the *Griswold*

decision relative to women in the same census region without these laws—even after adjusting for a host of observable characteristics.” (3) Interestingly, she finds no difference in the proportion of women who ever used barrier methods of contraception. She concludes that this difference is due to the increased cost of apprehension for doctors and pharmacists, who risk losing their license to practice, as opposed to more casual vendors who might sell condoms. These casual vendors were often gas stations or convenience stores, and condom sales were rarely a significant fraction of their business.

Even if the restrictive sales bans were not *perfectly* enforced, Bailey (2010) shows that they substantially raised the fixed cost of using the birth control pill in restrictive states, which may have led some women to cross into permissive states to obtain the Pill. Since prescriptions for the Pill require a yearly physical examination as well as frequent refills at pharmacies, obtaining oral contraception from another state would require frequent trips across the border, so women living further from permissive states would have faced higher travel costs, whereas women living closer to permissive states (or those directly on the border) would have faced smaller travel costs.⁶⁵

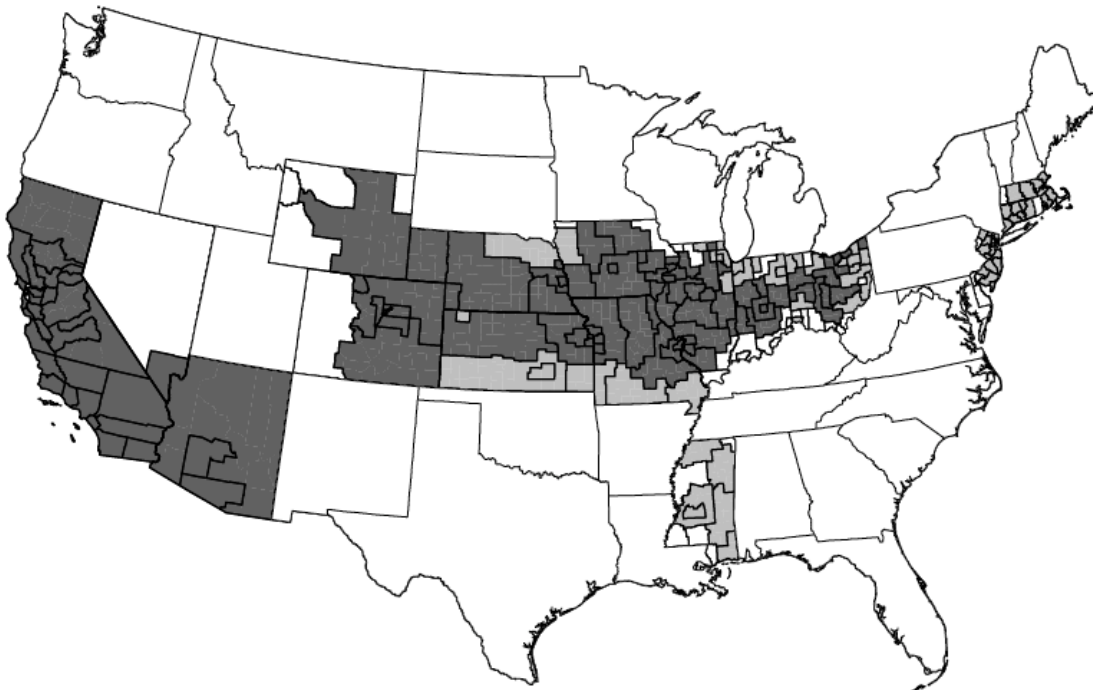
To test this hypothesis, we create a distance measure for each county group in the restrictive states in the 1970 and 1980 censuses.⁶⁶ We use the population-weighted centroid for each county group and measure the Euclidian distance from that centroid to the nearest border with a non-restrictive state. This is a noisy measure of the true distance required to access oral

⁶⁵ Due to the repeated cross-border trips required to obtain the Pill, we would expect that the difference in cost due to distance would be greater for the Pill than for abortion, which required only one cross-border trip.

⁶⁶ Some county groups are dropped in the 1970 sample. This is discussed in Section 4.4. Without question, using data at the county level rather than the county group level would be preferable, and we wrote a successful proposal to the U.S. Census Bureau to obtain access to the restricted county-level data, but circumstances beyond our control kept us from being able to complete our analyses with the restricted data.

contraception, since it is unlikely there were family planning clinics at every border, and roads to not run directly from one's home to the border. It is plausible, however, that women living relatively close to permissive states would have had little problem crossing the border to obtain a prescription for the Pill.

Figure 4.3: Geographic Distribution of County Groups by Distance Category (1970)



Notes: Light gray counties are part of county groups whose population-weighted centroid is less than 50 miles from a permissive border. Dark gray counties are part of county groups whose population-weighted centroid is more than 50 miles from permissive border. The within-state variation that we are using for our identification strategy comes largely from the Midwest—only Illinois, Indiana, Iowa, Kansas, Massachusetts, Missouri, Nebraska, and Ohio have county groups in both distance categories in 1970. Nevada is a restrictive state, but the majority of the state (outside of Las Vegas) is one county group that is split between Nevada and Utah, and thus dropped. Source: Authors' calculations using 1970 U.S. census shapefiles and the legal coding of Bailey (2010).

For simplicity, we define the treatment group to be women living more than 50 miles away from the closest non-restrictive border, and our control group to be women in restrictive states who live within 50 miles of a permissive border. Figure 4.3 shows the geographic

distribution of these distance categories using the county groups from the 1970 census.⁶⁷ It is worth noting that the restrictive states were distributed across the U.S. (including both California and Mississippi), and do not appear to reflect any particular political ideology of the 1960s. Both restrictive and permissive states are found in each of the four census regions. Unfortunately, many restrictive states have all their counties located in one distance band or another, so the within-state variation in the distance category is concentrated among a smaller subset of states, primarily in the Midwest.⁶⁸

Table 4.2 presents summary statistics by state and distance category. Overall, the sample size for states with variation is fairly balanced in 1970, though less so in 1980. There are only slight differences in average age, employment, poverty, or educational attainment between women in the two distance categories. Women in far counties are slightly more likely to be white, and slightly more likely to have graduated high school, but none of the differences are statistically significant. The results presented here are for only the subsample of women in states with variation in distance categories, but including all restrictive states does not meaningfully change things.

⁶⁷ Because the distances were calculated from the population-weighted centroid of each county group, there are several counties that are close to non-restrictive borders but are classified as being more than 50 miles away because the centroids of those groups are more than 50 miles from the border.

⁶⁸ Only Illinois, Indiana, Iowa, Kansas, Massachusetts, Missouri, Nebraska, and Ohio have county groups in both distance categories in 1970. In 1980, Arizona, Colorado, Mississippi, and Wyoming also have groups in both categories.

Table 4.2: Summary Statistics of Selected Variables*A: Sample Size (Female Population over Age 14) by State and Distance Group*

	1970		1980	
	d50=0	d50=1	d50=0	d50=1
Arizona		11,568	2,534	49,423
California		134,294		458,619
Colorado		13,476	4,308	50,707
Connecticut	20,363		62,036	
Delaware			11,681	
Illinois	46,515	27,453	149,894	72,687
Indiana	11,791	17,765	63,241	42,253
Iowa	2,883	13,277	14,899	40,517
Kansas	4,774	7,698	12,036	33,159
Massachusetts	32,650	3,061	113,003	2,951
Mississippi	11,742		38,762	8,908
Missouri	7,472	26,543	12,838	83,804
Nebraska	2,883	7,721	2,067	27,767
Nevada		3,428		15,404
New Jersey	47,995		147,782	
Ohio	33,243	31,431	174,710	35,409
Wyoming		2,142	1,949	6,376
Number of states with variation	8		12	
Total in states with variation	142,211	134,949	590,241	453,961
Total	222,311	299,857	811,740	927,984

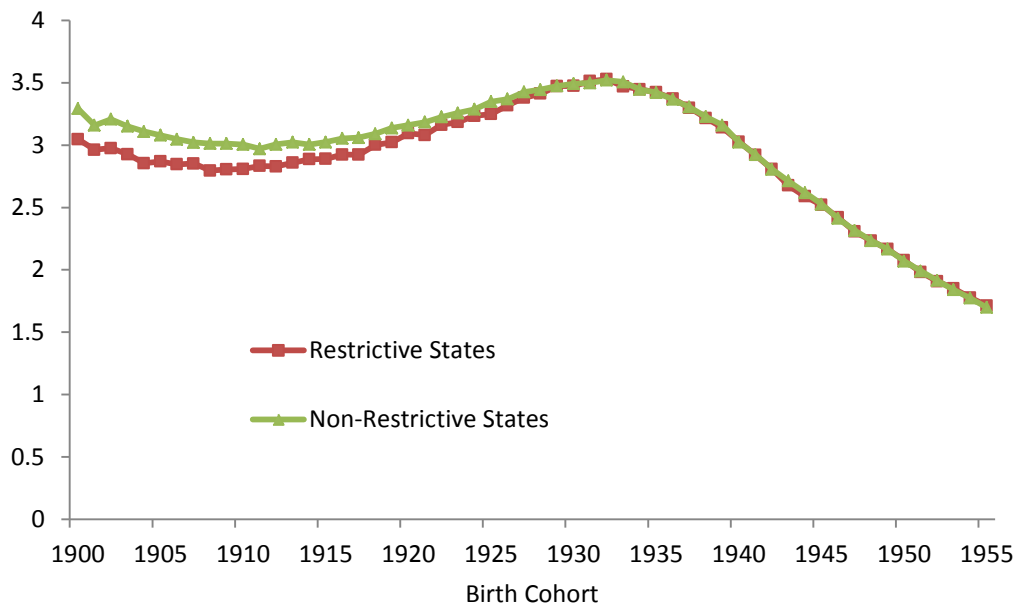
B: Other Outcomes for Women in States with Variation by Distance Group

	1970		1980	
	d50=0	d50=1	d50=0	d50=1
Average age	39	38	41	40
Currently married	64%	65%	56%	57%
% nonwhite	11%	9%	13%	11%
Currently employed	44%	43%	48%	49%
Foreign born	8%	7%	8%	10%
% in poverty	12%	13%	13%	13%
HS diploma	56%	61%	65%	69%
College graduate	8%	8%	11%	12%

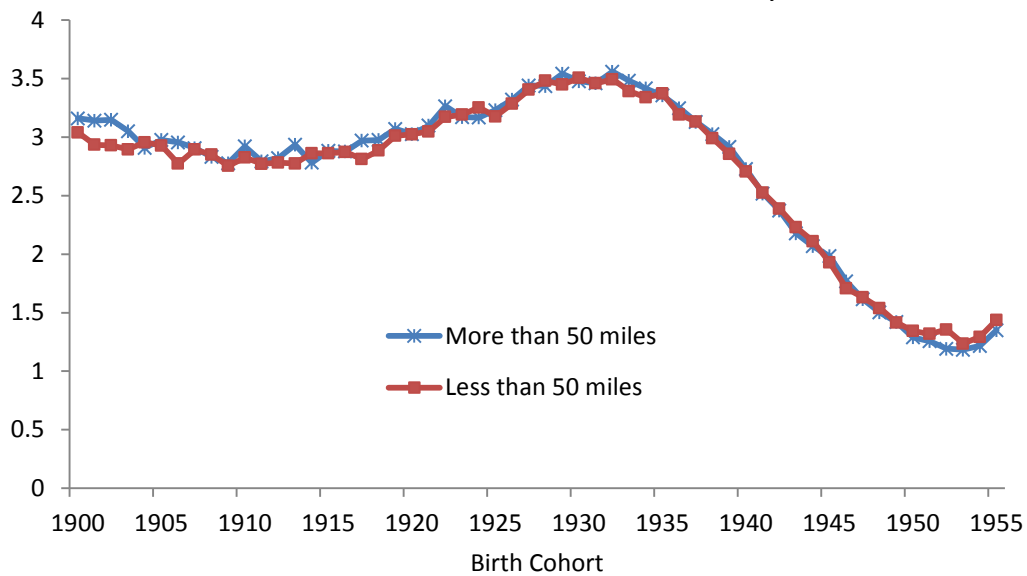
Restrictive states with county groups in both distance categories (variation states) are shaded. Source: 1970 and 1980 census (Ruggles et al. 2015)

Figure 4.4: Children Ever Born by Mother's Year of Birth

A. Restrictive vs. Non-Restrictive States



B. Within Restrictive States with Variation by d50



The figures plot the mean number of children ever born to mothers in permissive states, restrictive states (Panel A) and in restrictive states with variation (IL, IN, IA, KS, MA, MO, NE, OH) within and beyond 50 miles of a permissive border (Panel B). The sample excludes childless women. Source: 1980 census, 5% sample, IPUMS (Ruggles et al, 2015).

Panel A of Figure 4.4 plots the mean number of children ever born to mothers in restrictive and non-restrictive states by the mother's year of birth, using 1980 census data.

Women in non-restrictive states historically had more children than women in restrictive states, and the trends, while similar, follow different trajectories. This suggests that the demand for children was evolving differently in restrictive and non-restrictive states for those cohorts and calls into question empirical strategies that rely on the change in the difference in fertility outcomes between restrictive and non-restrictive states being driven solely by access to the Pill. Our empirical design tries to overcome this problem by using within-state variation. Panel B of plots the mean number of children ever born to mothers in restrictive states with variation who are within or beyond 50 miles of a non-restrictive border. The evolution of children ever born for these two groups follows a substantially similar path, suggesting that they make a more plausible comparison.⁶⁹

4.3 Theory and Hypotheses

Following Bailey (2010) and Michael and Willis (1976) we treat a couple's number of children as a random variable. Couples choose a contraceptive strategy that determines their monthly probability of conception. Choosing a contraceptive strategy is equivalent, under their assumptions, to choosing a distribution of the number of children defined by the mean, μ , and associated variance, σ^2 . Each contraceptive strategy j has an associated cost, F_j . The model employs a two-step decision-making process. First, for any given choice of (μ, σ^2) the couple selects the least costly contraceptive technique. Second, they select

⁶⁹ Note that this comparison does not control for differences in education, race, or income between near and far county groups as does the rest of our analysis.

the (μ^*, σ^{*2}) that maximizes their total expected utility of children, net of the costs of fertility control.⁷⁰

Contraceptive strategy j can be decomposed into two dimensions: (1) the choice of the contraceptive technique and (2) the care or intensity with which that technique is used. This implies that each strategy has both a fixed and a marginal cost component. Fixed costs include the time spent learning about a method or finding a provider. Marginal costs include any ongoing costs associated with use of the method, whether financial, time, or psychological. It is easiest to think of the marginal costs defined per sexual encounter, with longer-term costs such as prescription refills divided over the total number of encounters over the couple's fertile lifetime.

We let $F_j \equiv F_j(\mu) = \alpha_j + \beta_j(\mu_N - \mu)$, where μ_N is the mean number of children born in the absence of any contraception (the natural fertility rate) and $\mu_N - \mu$ is thus the desired number of averted births. Methods such as abstinence or withdrawal are likely to have high marginal costs associated with the behavioral costs involved in changing the frequency or act of coitus, but no significant fixed costs. Barrier methods such as condoms involve a small fixed cost of learning about the method and a marginal cost of the inconvenience of use at the time of coitus, plus some fraction of the cost of purchasing additional supplies (assuming supplies are not procured separately for each encounter).⁷¹ We assume that the marginal cost of condoms

⁷⁰ In this model, the couple chooses a contraceptive strategy defined by (μ^*, σ^{*2}) , so the choice of σ^{*2} is never independent of the choice of μ^* . As a result, we cannot separate the effect of μ from the effect of σ^2 on fertility choices.

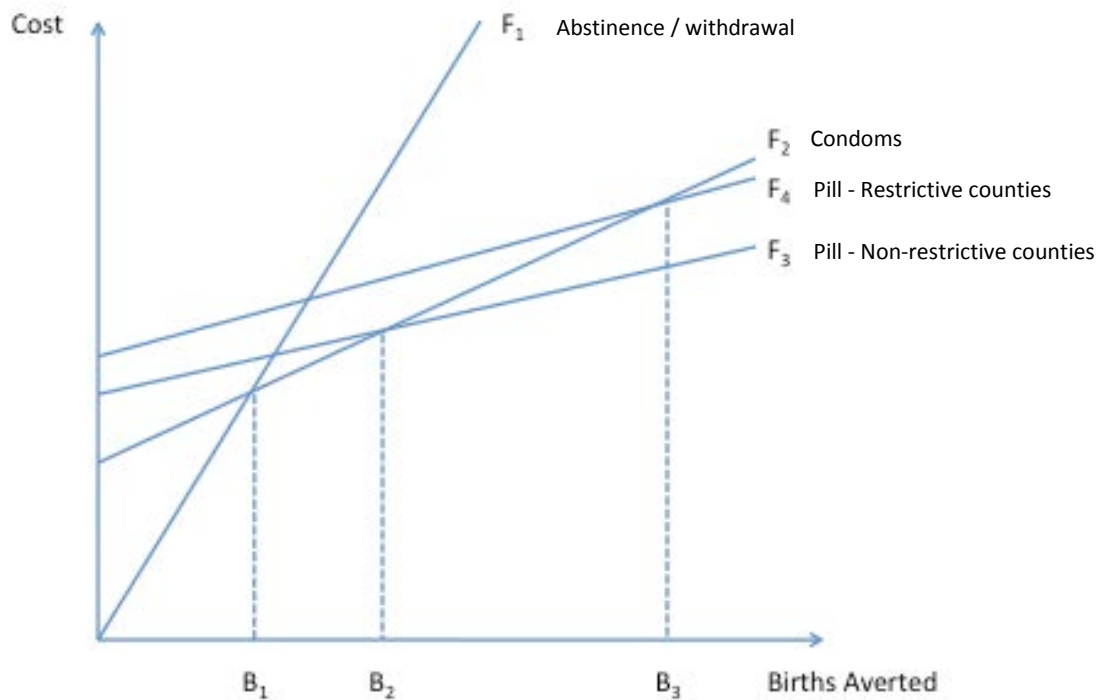
⁷¹ The choice of a diaphragm would likely involve comparable fixed costs to the Pill, because it also requires finding a physician to prescribe it, as well as significant learning costs to use it effectively. The marginal costs of a diaphragm would likely be higher due to the inconvenience at the time of intercourse. This example is omitted for simplicity.

is less than the marginal cost of abstinence or withdrawal. The fixed costs of the Pill will be higher than those of condoms, since the Pill requires finding a physician to prescribe and a pharmacist to fill it, as well as significant costs associated with learning about the method and weighing potential side effects. The marginal costs of the Pill, however, are relatively small, and consist primarily of some fraction of the cost of prescription refills and the psychic costs involved in remembering to take a pill every day. This concept of fixed and marginal costs for different types of contraceptive strategies is plotted in Figure 4.5. F_1 is the cost associated with abstinence or withdrawal, F_2 is the cost associated with condoms, and F_3 is the cost associated with the Pill. If the number of desired averted births, $\mu_N - \mu$, is less than B_1 , the lowest cost method will be withdrawal or abstinence. If $B_1 < \mu_N - \mu < B_2$, the lowest cost method will be condoms, and if $\mu_N - \mu > B_2$, the couple will choose to adopt the Pill.

Living more than 50 miles away from a permissive border may raise both the fixed and marginal costs of the Pill. Women face fixed psychic costs of breaking the law, in addition to the increased travel costs involved in obtaining both the initial prescription and the frequent refills. This effectively raises the cost to F_4 for those women. It is clear that the threshold for adoption of the Pill, in terms of number of desired averted births, will increase, in this case from B_2 to B_3 . As a result, we expect that fewer women will adopt the Pill in more distant counties and those that do will be women who want to avert high numbers of births, particularly younger women.⁷²

⁷² Since the natural fertility rate, μ_N , is decreasing in fertility and age, for any given level of μ , a younger woman will have a larger value of $\mu_N - \mu$ because her μ_N is larger.

Figure 4.5: Costs of Different Contraceptive Strategies



Notes: F_1 corresponds to abstinence or withdrawal, F_2 to barrier methods such as condoms, F_3 to the Pill in or near a non-restrictive state, and F_4 to the Pill in a non-restrictive state.

We hypothesize that women without access to the Pill during the period of 1957 to 1965 would have more children than women with access due to the increased cost of averting births. We would expect these effects to be more highly concentrated among younger women, since younger women likely desire to avert larger numbers of births.⁷³

⁷³ Additionally, Bailey (2010) found that the strongest impact of the birth control pill on birth rates was among women in their 20s and early-30s.

4.4 Data and Estimation Results

4.4.1 Period fertility

We use county-level vital statistics collected as part of Bailey's ongoing project, "Family Planning Programs and the Health and Fertility of US Women, 1960-1980" (Bailey 2012) to examine the impact of distance to a permissive state on period fertility rates.⁷⁴ We calculate general fertility rates by dividing each county-by-year birth count by $1/1000^{\text{th}}$ of the relevant population of women from the published census files in decennial census years and linearly interpolating in intercensal years. We use the same methodology to generate covariates from census data including the percent of the county population that is classified as urban or nonwhite, the median income for the county, and the percent of the population over 18 that graduated from high school.

To capture the effect of distance, we calculate the distance from the geographic centroid of each county to the closest permissive border, and generate an indicator variable $d50$ that is equal to one if that distance is greater than or equal to 50 miles. We break the distance at 50 miles because this seems a prohibitive distance for women to travel regularly. A distance of 50 miles would require at least two hours of round-trip commuting time, and likely significantly more. We believe this time (and fuel/fare) cost was likely prohibitive for most women in 1960, especially given that the trip would need to be made frequently to refill the prescription, so that women beyond this threshold had very high costs to use the Pill.⁷⁵

⁷⁴ These data are at the individual county level, unlike the census data, which are at the county-group level.

⁷⁵ We also explored several alternative distance specifications including log distance and both 20 and 70 mile thresholds, all of which provided substantially similar results.

To explore the impact of distance to a permissive border on the period fertility effects described in Bailey (2010), we replicate those regressions using the county-level data. Following Bailey (2010), we estimate

$$GFR_{c(s)t} = Sales_s f'_t \tau_1 + Exception_s f'_t \tau_2 + Advertising_s f'_t \tau_3 + g_r f'_t + h_s + X'_{ct} + \varepsilon_{ct} \quad (4.1)$$

where $GFR_{c(s)t}$ is the general fertility rate for county c in state s and year $t=1950$ to 1980 ;

$Sales_s$ indicates if state s had a sales ban; $Exception_s$ indicates if the state had a Physician exception; $Advertising_s$ indicates if the state had an advertising ban; f'_t is a set of year fixed effects plus a constant; $g_r f'_t$ is a set of region-by-year fixed effects; h_s is a set of state fixed effects; and X'_{ct} includes time-varying county-level covariates. The point estimates of interest are the coefficients on the interaction of $Sales$ and year, τ_1 , which capture changes in the difference in birth rates in states with restrictive sales bans relative to those in other states in the same census region.⁷⁶

The analog of regression (4.1) using our measure of distance is

$$GFR_{c(s)t} = d50_c f'_t \beta_1 + g_r f'_t + h_s + X'_{ct} \delta + \varepsilon_{ct} \quad (4.2)$$

where $d50=1$ if the center of county c is 50 miles or more from a permissive border, and 0 otherwise; all other variables are as previously defined. In this specification, β_1 represents changes in the gap in birth rates in counties far from permissive borders (far counties) relative to those in counties that are close to permissive borders or in permissive states in the same census region. In both specifications, standard errors are clustered at the county level to correct for serial correlation within counties (Arellano 1987).

⁷⁶ These results are very similar but not exactly identical to those in Bailey (2010) because of slight differences in the county-level data and slightly different covariates. Additionally, Bailey (2010) classified births by the mother's state of birth, whereas here they are classified by the mother's county of residence (county of mother's birth is not reported).

Panel A of Figure 4.6 graphs estimates of τ_1 and β_1 along with 95 percent confidence intervals, showing that birth rates in areas where the Pill was harder to obtain increased more rapidly between 1957 and 1965 relative to areas where it was less costly. Importantly, moving from the state-level variation in Bailey (2010) to county-level variation in distance to a permissive border provides a largely similar though slightly attenuated effect, suggesting that most of the between-state variation in Bailey (2010) is driven by women living more than 50 miles from a permissive border and captured by $d50$. The distance specification generates larger standard errors than Bailey's, likely because the treatment group is 30 percent smaller (983 counties in restrictive sales ban states compared to 616 counties 50 miles or more from a permissive border).

Because the distance specification exploits within-state variation in the cost of obtaining the Pill, we can alter equation (4.2) to include state-by-year fixed effects to capture unobserved time-varying policy changes that varied between states, such as changes in state-level funding for family planning clinics, as well as unobserved attitudinal changes between states:

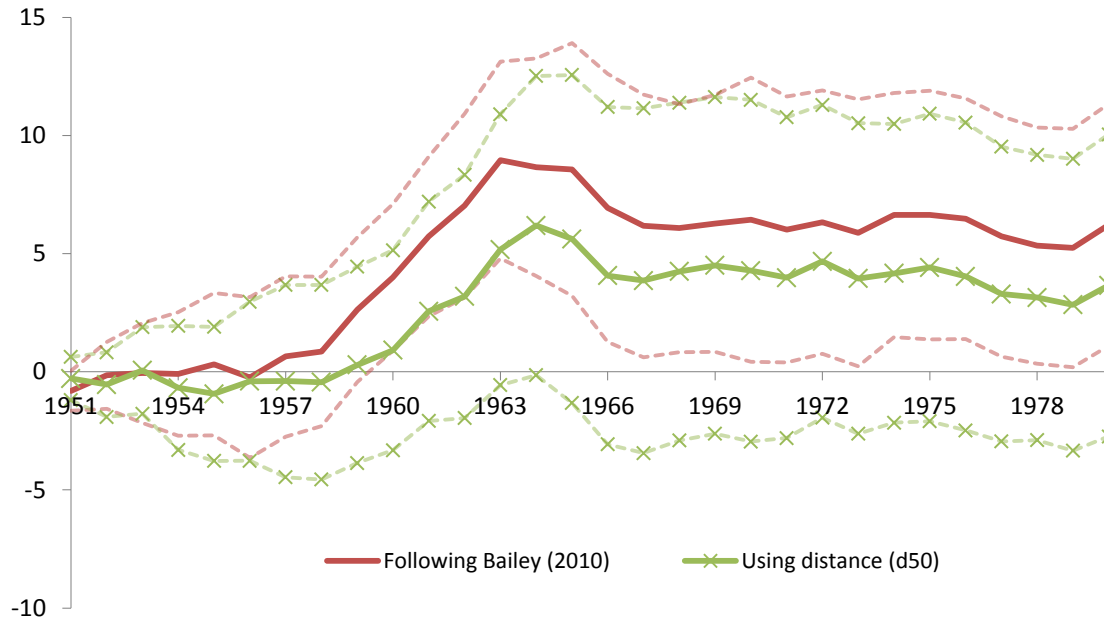
$$GFR_{c(s)t} = d50_c f'_t \beta_2 + h_s f'_t + X'_{ct} \delta + \varepsilon_{ct} \quad (4.3)$$

In this specification, the coefficients of interest, β_2 , represent changes in the gap in birth rates in far counties relative to near counties in the same state and year, and are graphed in Panel B of Figure 4.6.⁷⁷ Strikingly, the effects in Panel A disappear with the inclusion of state-by-year fixed effects, suggesting that the increased cost of obtaining the Pill induced by a distance of more than 50 miles did not have a substantive effect on fertility rates, when compared to areas

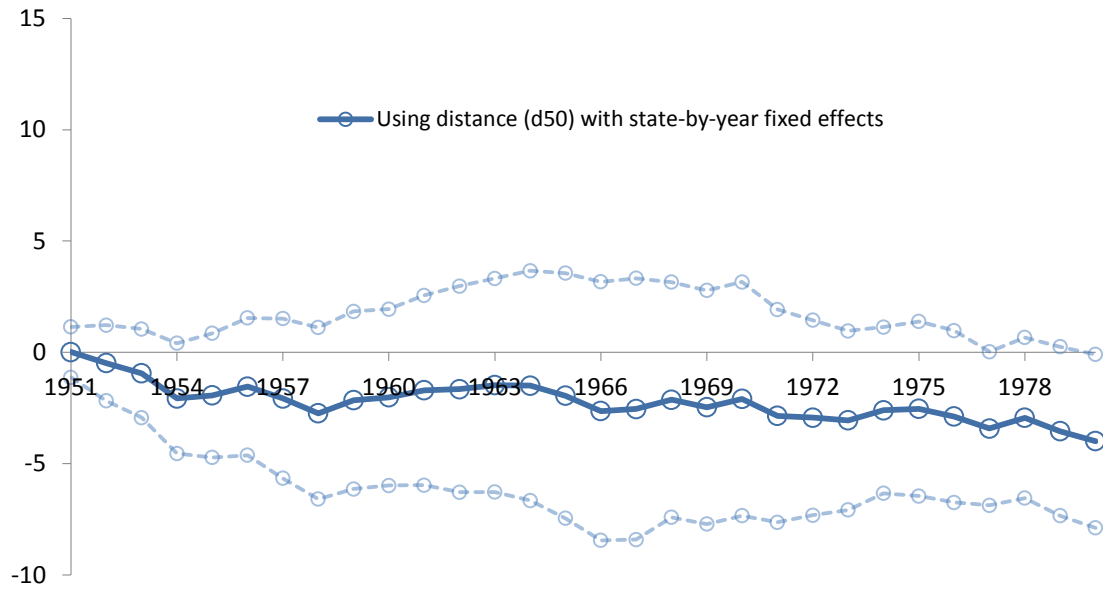
⁷⁷ Importantly, variation in distance categories exists in all restrictive states in the Vital Statistics data, because it is at the county level, rather than county group.

Figure 4.6: Differential Evolution of Period Fertility Rates

A. Specifications with Region-by-Year Fixed Effects



B. Specification with State-by-Year Fixed Effects



Notes: The figure presents point estimates for τ_1 and β_1 from regressions (4.1) and (4.2) in Panel A, and β_2 from regression (4.3) in Panel B, with county-level general fertility rate as the dependent variable. 95% confidence intervals are shown. Each series is a separate regression. Panel A represents the change in the gap in period fertility rates between restrictive sales ban states and those in permissive states in the same census region; and the difference between counties far from permissive borders and those closer to or in permissive states in the same census region. Panel B presents the change in the gap in period fertility rate between counties far from permissive borders and those closer to permissive states in the same state. Source: Authors' calculations with vital statistics data from Bailey (2012).

with less burdensome distances to obtain the Pill. This could indicate that the variation in Panel A was driven by other state-level policy changes that were not related to distance from permissive borders, or that the cost differential induced by the increased distance to a permissive border was not substantial enough to generate measurable differences in birth rates.

4.4.2 Completed fertility

The 1970 and 1980 decennial censuses, made available through the Integrated Public Use Micro Samples (IPUMS), provide data for our analysis of completed fertility effects. The smallest geographic region in the publicly available census data is county group, a collection of generally (though not always) contiguous counties that contain at least 250,000 people. Unfortunately, some of the county groups in the 1970 census cross state lines, which is problematic for our analysis.⁷⁸ If the county group crosses a border between two permissive or two restrictive states, we include the group and classify it as belonging to the state with the majority of the county group's population. County groups that cross restrictive/permissive borders are more problematic, because we cannot always determine the appropriate legal regime for those women. To include as many women as possible in our analysis, we classify county groups that have 80 percent or more of their population in one state as belonging to that state.⁷⁹ County groups that are more evenly split between restrictive and permissive states are dropped from the analysis. The dropped counties make up roughly 7 percent of the 1970 sample by population and are concentrated in the Northeast.

⁷⁸ The 1980 county groups do not cross state lines, so this is not an issue in the 1980 sample.

⁷⁹ This misclassifies up to 20 percent of the women in the group. Dropping all the groups that cross restrictive/permissive borders does not materially change the results but increases the standard errors. The included restrictive/permissive groups represent less than 2 percent of the 1970 population.

For each restrictive county group, we calculate the distance from the population-weighted centroid of the county group to the closest permissive border. We define d_{50} to be equal to one if that distance is 50 miles or more, and zero otherwise. Only eight of the restrictive states have county groups that are both within and beyond 50 miles from a non-restrictive border in the 1970 census, so the effects are primarily identified off of those eight states.⁸⁰

We classify women by their county group of residence at the time of the census. It is possible that county group of residence is endogenously related to the presence of sales bans, if those women who were more likely to use contraception moved to non-sales ban states in response to their desire for easy access to the birth control pill.⁸¹ We address this by running our analysis on the sample of women who have not moved in the last five years, which barely changes the estimates. It is worth noting that as time passes women are less likely to be observed in the county group where they resided between 1957 and 1965, so this source of measurement error may be larger for the 1980 sample.

Women are grouped into three-year birth cohorts. The 1915-1917 cohort is omitted in all regressions. This cohort makes a plausible comparison group, since these women would have been 40 to 43 in 1957 when *Enovid* was first introduced, and unlikely to have been fertile. Following the demography literature, we define women's reproductive ages to be 15-44. Women who would have been fertile both at the introduction of *Enovid* in 1957 and at the

⁸⁰ The states that have county groups in both distance bands in 1970 are Illinois, Indiana, Iowa, Kansas, Massachusetts, Missouri, Nebraska, and Ohio. In 1980, Arizona, Colorado, and Wyoming also have county groups in both distance bands.

⁸¹ We also ran a similar analysis classifying women by their state of birth, which is almost certainly exogenous, and their state of residence at the time of the census and get substantially similar results.

Griswold decision in 1965 would have been born between 1921 and 1942, so we expect the effects to be concentrated among those cohorts. Effects on women born as early as 1917 or as late as 1950 would still be plausible, however.

For fertility outcomes in the census data, we estimate a slightly different version of equation 4.3:

$$X_{g(s)y} = d50_g f'_y \beta_3 + h_s f'_y + X'_{gy} \delta + \varepsilon_{gy} \quad (4.4)$$

where $X_{g(s)y}$ is an outcome for birth cohort y in county group g in state s , $h_s f'_y$ is a full set of state-by-birth-cohort fixed effects, and X'_{gy} is a set of covariates for each county group and birth cohort cell, including the proportion of 15 to 44 year old women who reside on a farm, live in poverty, are currently married, and are nonwhite, and mean educational attainment. The coefficients of interest, β_3 , represent the change in the difference of a given outcome for women living far from a permissive border relative to those in the same birth cohort in the comparison group, and represent the impact of the increased cost associated with obtaining the Pill for women living far away from permissive states. Because we are using within-state variation, all static and time-varying differences between states (such as access to abortion or cultural norms) are absorbed by the fixed effects.

Throughout our analysis, we use three comparison groups. The first, “restrictive states,” compares all women in far ($d50=1$) county groups to women in near ($d50=0$) county groups across all seventeen restrictive states. In this first comparison, we are comparing the differential evolution of a given outcome between women living far from a permissive border relative to those in the same birth cohort living near to a permissive border. The second, “variation states,” restricts the sample to only the states that have county groups in both

distance categories (eight states in 1970, twelve states in 1980), to exclude any influence from states such as California that only have county groups in one band. This is our preferred specification. Our third comparison group “variation + border,” restricts the treatment group to only those states that have county groups in both distance categories, but expands the control group to include immediately adjacent county groups in permissive states just on the other side of the border from states with variation. This assumes that women close to borders behave similarly, regardless of which side of the border they live on. In general, all three comparison groups provide very similar results. We also ran all analyses on ever-married women and women who had not moved between states in the last 5 years, neither of which meaningfully changed the point estimates or standard errors.⁸²

There are two possible mechanisms through which access to the Pill could affect women’s fertility. The first mechanism is by changing the total number of births born to women with easy access. We might expect that women without access to the pill only had imperfect means of controlling their fertility and might have more children if their contraceptive strategies failed more frequently. Alternatively, if the risk of overshooting the desired number of children was large in the absence of reliable contraception, access to the Pill could have increased total family size, because couples would have felt more comfortable closely approaching their true desired family size. The other mechanism is one of timing. It is also plausible that the demand for children remained constant across access to the Pill, but that women with access were able to have their children later and space them more appropriately.

⁸² We also ran the same analyses on the entire sample, classifying all permissive states in the same category as near county groups, with very similar results.

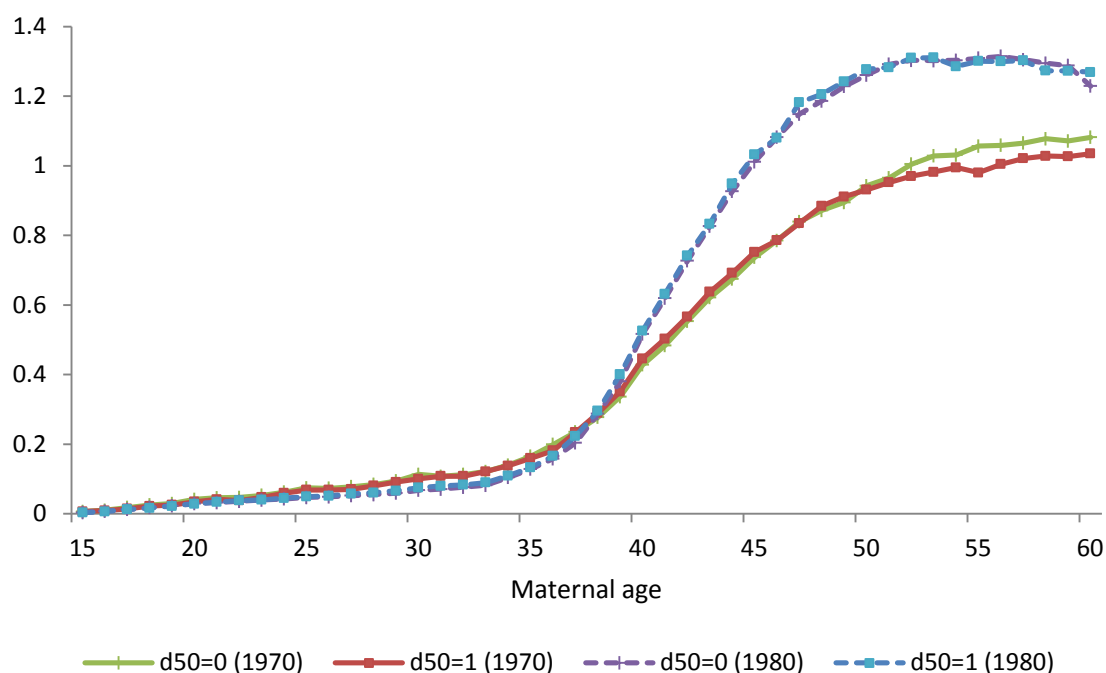
In states with sales bans, women might have their children earlier and then adopt more permanent means of contraception once their desired number of children had been reached.

To examine the timing effects of restrictive sales bans, we break children ever born into three groups: those born prior to 1958, whose conceptions would have occurred prior to the introduction of *Enovid* (*Pre58*); those born between 1958 and 1966 (inclusive), who were conceived when the sales bans were in full effect (*P58_66*); and those born after 1966, who were conceived following *Griswold* (June 1965, *Post66*). *Post66* is the count of the woman's children in the household who were born in or after 1966. *P58_66* is the count of her children in the household born in the years 1958 to 1966. Both of these measures will understate the true number of children if there are children who are no longer in the household. In 1970, *Post66* and *P58_65* children would have been under 4, or between 4 and 12 years old, respectively, and highly unlikely to have left the household (other than by death). *Pre58* children, however, would have been 13 or older, and substantially more likely to have left home. For that reason, *Pre58* is calculated by subtracting *Post66* and *P58_66* from children ever born, which would include any deceased children and children who left home before 13.⁸³ For robustness, we also present results using the reported number of children in the household, though we prefer the calculated version. Importantly, because we use a difference-in-differences framework, this bias is only an issue if children die or leave home at different rates that depend on the distance to a permissive border.

Figure 4.7 examines this issue explicitly by comparing the average number of missing children, defined as children ever born minus the number of children in the household, by

⁸³ *Pre66* is calculated similarly by subtracting *Post66* from children ever born.

Figure 4.7: Mean Children Not in Household by Maternal Age and Distance

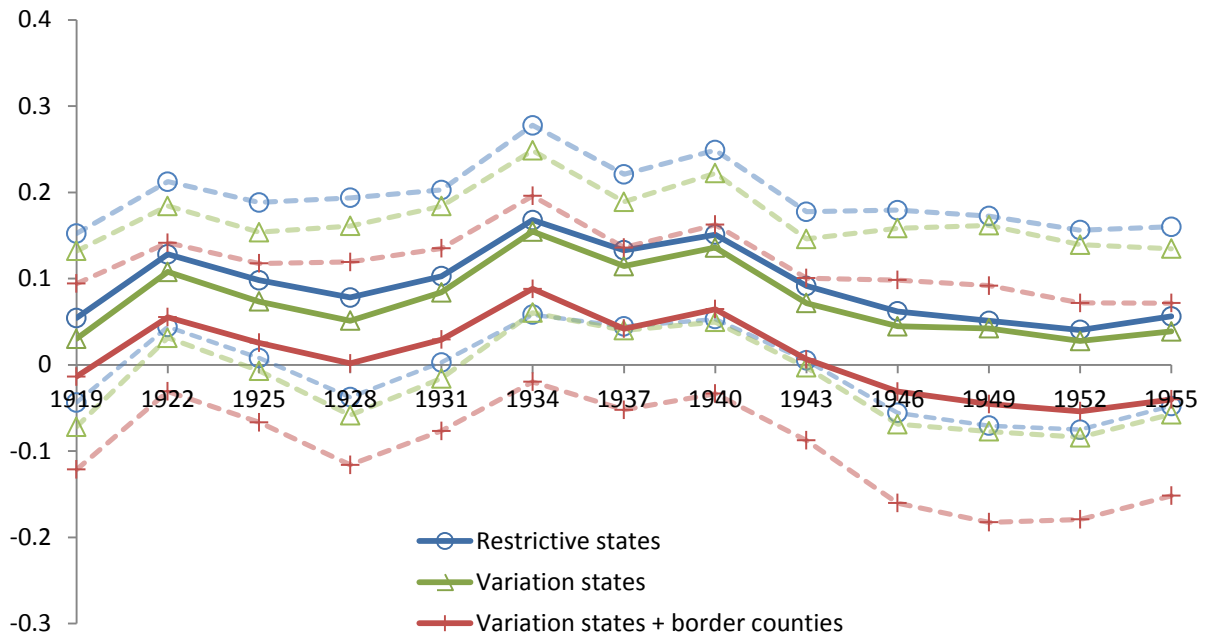


Notes: The figure plots the average difference between children ever born and reported children in the household by maternal age and distance of the county group to the nearest permissive border (d50) in the 1970 and 1980 censuses. Source: 1970 census, metro forms 1 and 2; 1980 census, 5% sample; IPUMS (Ruggles et al, 2015).

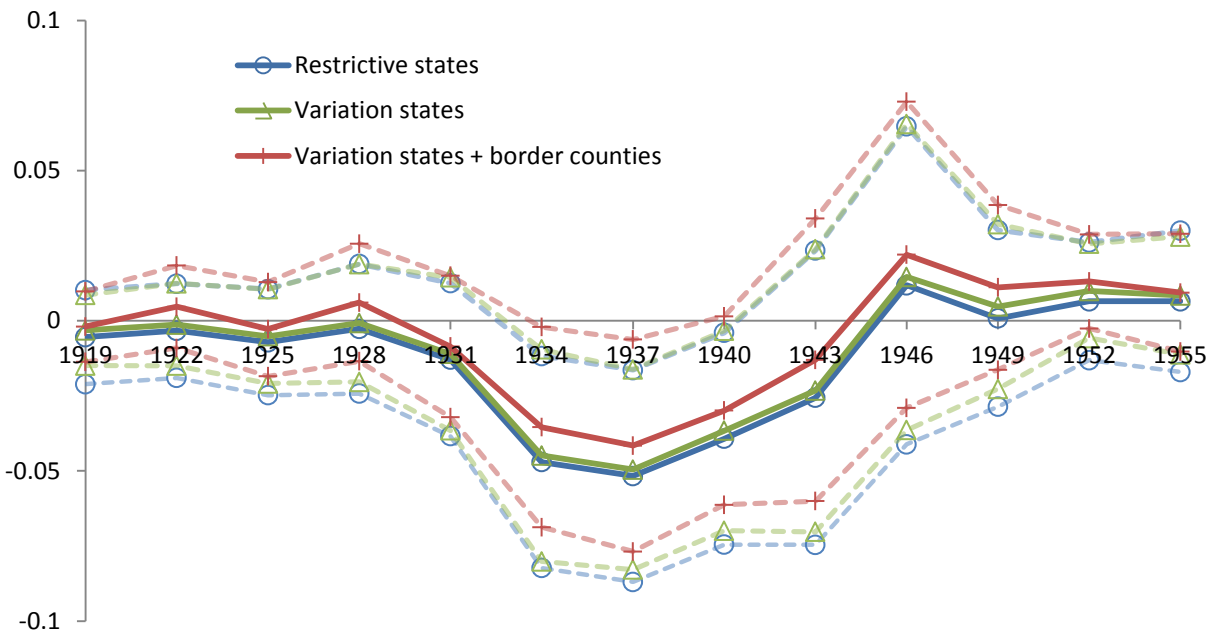
maternal age and d50 in both the 1970 and 1980 censuses. While it appears that substantially more children have left the household in 1980 for a given maternal age, the only difference between distance categories is among women over 52 in 1970—women born prior to 1918 in near county groups seem to have more of their children missing from the household than women of a similar age in far county groups. This implies that the calculated *Pre58* and *Pre66* values could be overstated for women in near counties born prior to 1918, while the reported versions would be similarly understated. Because we use the 1915-1917 birth cohort as our comparison group, this difference could bias our estimates, though the calculated and reported measures should be biased in opposite directions.

Figure 4.8: Differential Evolution of Children Born Before and After 1966, by Cohort

A. Children Born Before 1966



B. Children Born After 1966



Notes: The figure plots β_3 coefficients on d50 interacted with birth cohort dummies from regression (4.4) for three groups: women in all restrictive states, women in restrictive states with variation in distance categories, and women in states with variation and women in immediately adjacent permissive border counties. Dependent variables are *Post66*, the number of children in the household born in 1966 or later, and *Pre66*, defined as children ever born minus *Post66*. The year on the horizontal axis is the midpoint of the 3-year birth cohort. Source: 1970 census, metro forms 1 and 2, IPUMS (Ruggles et al, 2015).

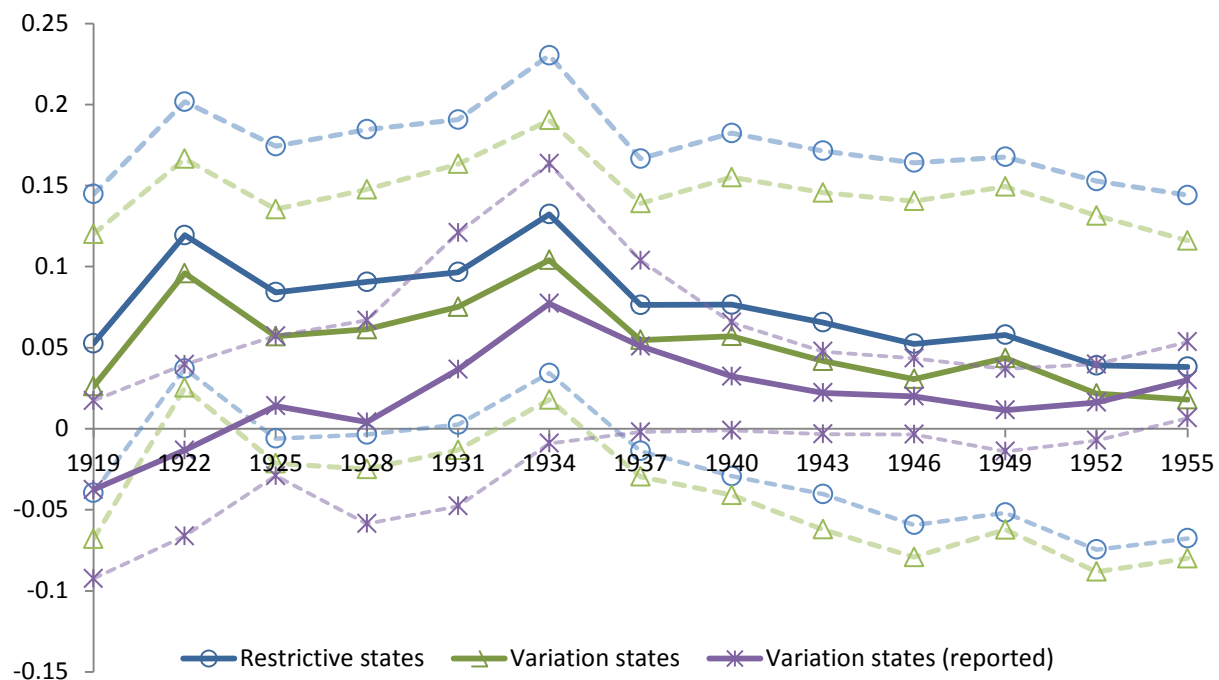
Figure 4.8 plots the β_3 point estimates for regression (4.4) using children born before and after 1966 as the dependent variables for our three comparison groups: all restrictive states, only those states with variation, and states with variation plus immediately adjacent permissive border counties. In panel A, looking at children born before 1966, all three comparison groups generate very similar patterns, though the estimates are substantially decreased when we include the immediately adjacent border counties, suggesting that prior to 1966 women in permissive counties adjacent to variation states did not behave entirely similarly to women on the other side of the border. In panel B, looking at children born after 1966, however, the comparison group including adjacent border counties behaves very similarly to the other two. With the exception of *Pre66* and *Pre58*, the results of comparison groups including the immediately adjacent border counties are largely indistinguishable from those of the variation states alone.

We find that among the birth cohorts from 1933 to 1941, women living more than 50 miles away from a permissive border had fewer children prior to 1966 and more children after 1966, relative to women in the same cohort and state who lived near to a permissive border. This would seem to imply that women in those cohorts, who would have been 16 to 24 when *Enovid* was introduced and 24 to 32 at the time of *Griswold*, had relatively more children when their access to the Pill was restricted (0.135 additional children, or 8 percent of the mean 1.74 children before 1966), and then compensated by having fewer children once the Pill became available (0.04 fewer children, nearly 15 percent of the mean of 0.276 children after 1966). This would suggest that the primary effect of access to the Pill is one of timing—women with

restricted access to the Pill brought their childbearing forward and had more children at a younger age than their counterparts with easy access.

When we decompose *Pre66* children further, into those born before 1958 and those born between 1958 and 1966, the picture becomes murkier. Figure 4.9 again presents β_3 point estimates, but with the calculated number of children born in 1958 or earlier as the dependent variable (children ever born minus the total number of children in the household born after 1958), as well as the reported number of children born prior to 1958, to account for the potential measurement error in the calculated version.⁸⁴

Figure 4.9: Differential Evolution of Children Born Before 1958, by Cohort



Notes: The figure plots β_3 coefficients on d50 interacted with birth cohort dummies from regression (4.4) for two groups: women in all restrictive states, women in restrictive states with variation in distance categories. Dependent variables are *Pre58*, the calculated difference between children ever born and the number of children born in or after 1958, and the reported number of children born prior to 1958 in the household. Source: 1970 census, metro forms 1 and 2, IPUMS (Ruggles et al, 2015).

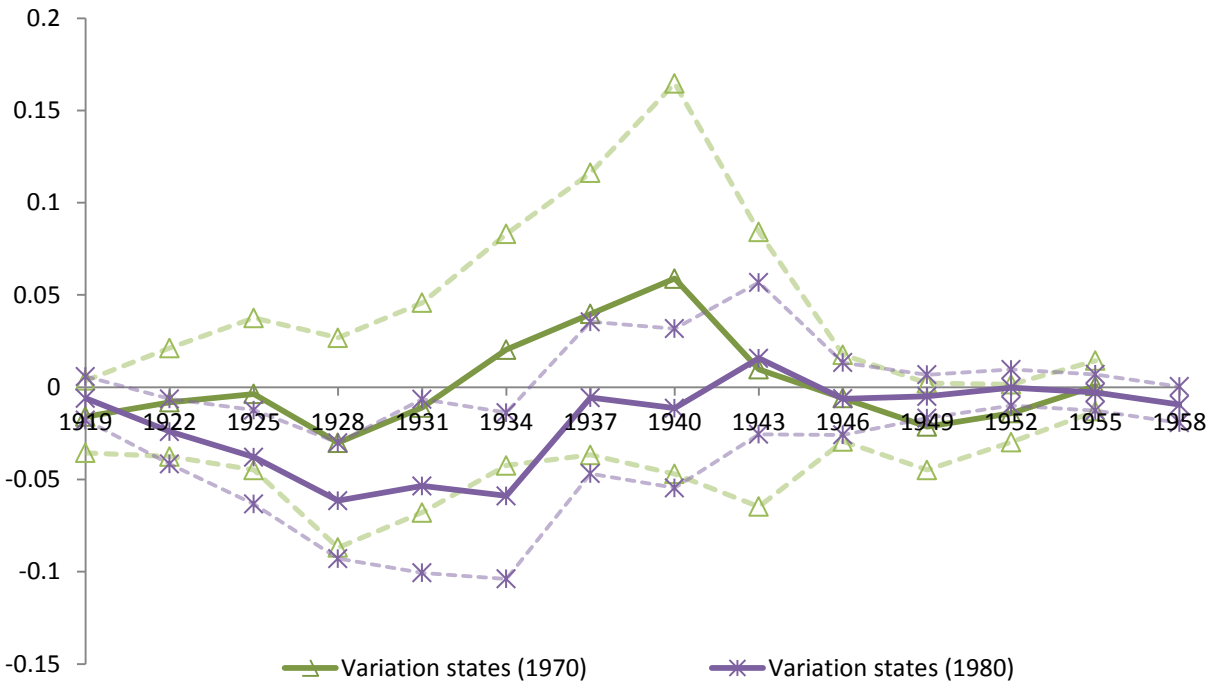
⁸⁴ For simplicity, this figure omits results from the variation plus adjacent border county comparison group—similarly to panel A of Figure 4.7, they follow the same pattern as the variation state results, just reduced by around 0.06 children across all cohorts.

If the differences in children born prior to 1966 were driven by differences in the cost of obtaining the Pill, we would not expect to see effects on children born prior to the introduction of the Pill in 1957. But it appears that women in county groups far from permissive borders were having more children than those living closer even before those borders were meaningful. In particular, the 1933-1935 birth cohort had 0.10 additional children by 1958, which accounts for 75 percent of the 0.15 additional *Pre66* children for that cohort in Figure 4.8. Figure 4.7 suggests that the calculated version of *Pre58* is perhaps biased upward for women in county groups close to permissive borders, which would be expected to bias our estimates of the difference downward if at all. As a robustness check, we also present estimates using the recorded number of children in the household born prior to 1958, which should be biased downward for the same reason, suggesting that the truth is somewhere between the two measures. Despite their smaller standard errors, the recorded estimates are not statistically different from zero, though they provide additional suggestive evidence that childbearing was measurably higher in county groups far from permissive borders prior to the introduction of the birth control pill.

Figure 4.10 looks specifically at children born between 1958 and 1966, when the sales bans would have had the most impact, plotting the β_3 point estimates with the number of children in the household born between 1958 and 1966 as the dependent variable. For brevity and legibility, we only present results for restrictive states with variation in both 1970 and 1980.⁸⁵

⁸⁵ The results for all restrictive states and variation states plus immediately adjacent border counties are virtually indistinguishable from those presented for variation states in both census years.

Figure 4.10: Differential Evolution of Children Born Between 1958 and 1966, by Cohort



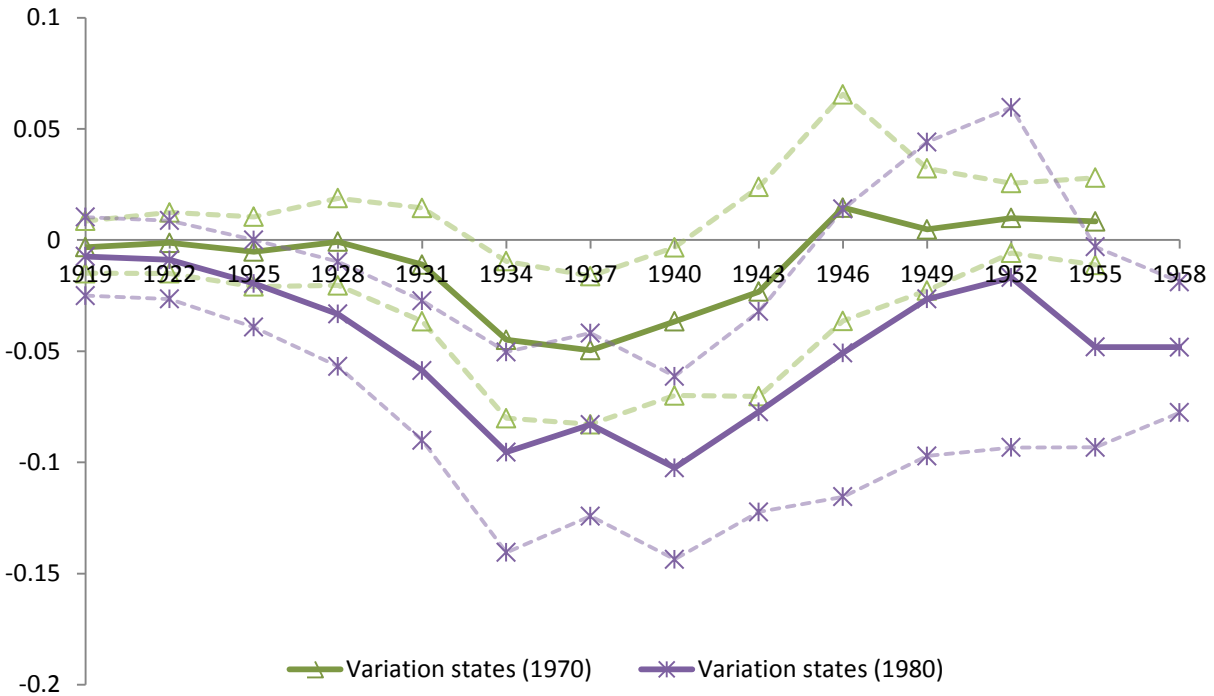
Notes: The figure plots β_3 coefficients on d50 interacted with birth cohort dummies from regression (4.4) for women in restrictive states with variation in the 1970 and 1980 censuses. Dependent variable is *P58_66*, the number of children born between 1958 and 1966 (inclusive) in the household. Each series is a separate regression, all including covariates. 95% confidence intervals are shown. The displayed year is the midpoint of the 3-yr birth cohort. Source: 1970 census, metro forms 1 and 2; 1980 census, 5% sample; IPUMS (Ruggles et al, 2015).

We find no statistically significant increase in births during the time the sales bans were in full force. There is suggestive evidence in 1970 that women in far county groups in the 1937 and 1940 cohorts may have had slightly more children than those in county groups closer to permissive borders, but the standard errors are very large and β_3^{1937} and β_3^{1940} are not jointly different from zero [$p=0.41$]. When we look at the same measures in the 1980 census data, it appears as though any effect of increased distance from permissive borders for the 1937 and 1940 cohorts has essentially vanished. Since the number of children born between 1958 and 1966 should be static in both 1970 and 1980, any difference is likely driven by changes in the data. In particular, because the 1980 county groups do not cross state borders, the 1980 sample

size is larger and four additional states have variation in their distance measures. Additionally, by 1980 children born between 1958 and 1966 would have been 14 to 22 years old and much more likely to have left home. It is unlikely that there was differentially greater attrition among children in far counties between 1970 and 1980, but even without differential attrition the increased measurement error likely biases our point estimates toward zero, and making it more difficult to find an effect. Together these results suggest that during the period when the sales bans were maximally impactful, the impact of distance to a permissive border did not translate into substantial differences in births. This is surprising but in keeping with the results from Panel B of Figure 4.4 that found no impact of distance on period fertility rates after the inclusion of state-by-year fixed effects.

Interestingly, in all specifications in all years, we do find statistically significant decreases in children born after 1966 to women in far counties relative to their counterparts closer to permissive borders. These results are graphed in Figure 4.11 and show that women born between 1933 and 1944 had fewer children in far counties than in counties close to permissive borders, both before and after 1970. By 1980, the effects of distance to a permissive border have increased and are significant for a larger range of cohorts, suggesting that the effect is long-lasting. The difference of 0.04 fewer children on average for the 1933 to 1944 cohorts in far counties in the 1970 census more than doubles to 0.09 fewer children in 1980, or 13 percent of the mean 0.687 children born between 1966 and 1980 in variation states. This would seem to suggest that the impact of distance to a permissive border was perhaps even more important between 1970 and 1980 than it was between 1957 and 1965.

Figure 4.11: Differential Evolution of Children Born After 1966 (1970 and 1980), by Cohort

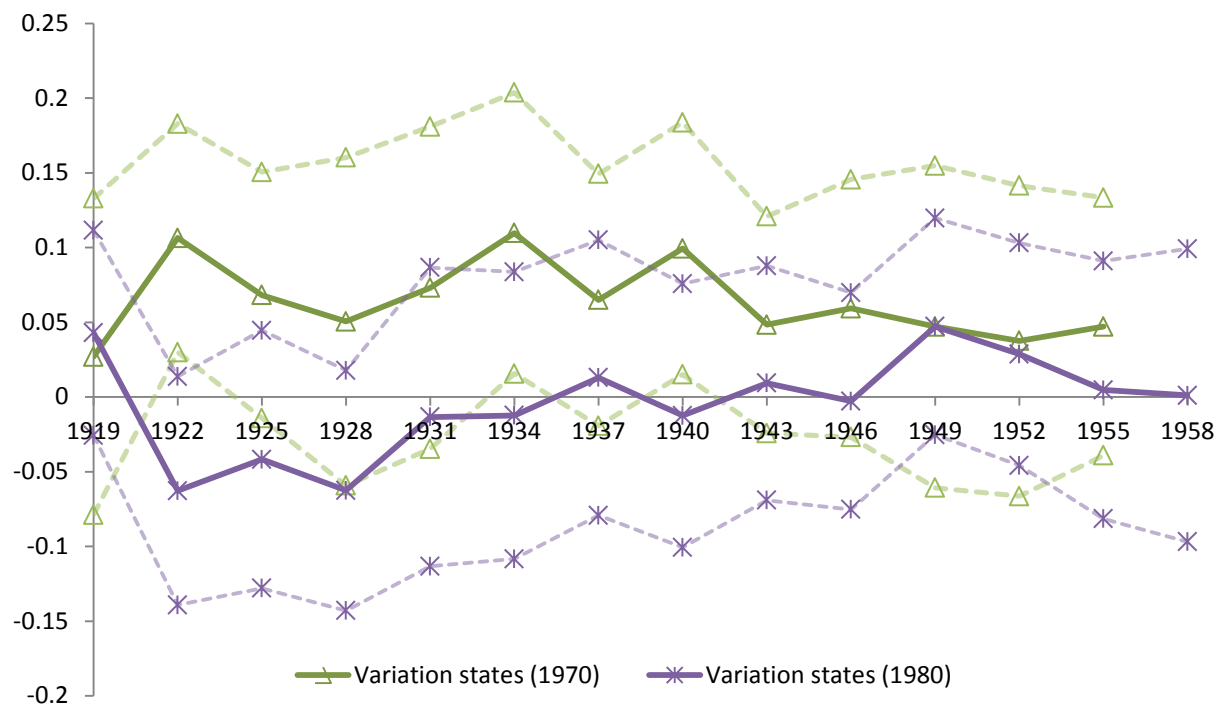


Notes: The figure plots β_3 coefficients on d50 interacted with birth cohort dummies from regression (4.4) for women in restrictive states with variation in the 1970 and 1980 censuses. Dependent variable is *Post66*, the number of children born after 1966 in the household. Each series is a separate regression, all including covariates. 95% confidence intervals are shown. The displayed year is the midpoint of the 3-yr birth cohort. Source: 1970 census, metro forms 1 and 2; 1980 census, 5% sample; IPUMS (Ruggles et al, 2015).

Indeed, looking at total children ever born in both the 1970 and 1980 census, there is no significant difference among women living more than 50 miles from a permissive border and their closer counterparts. Figure 4.12 plots the point estimates from regression (4.4) with children ever born as the dependent variable. We find that there is no significant trend in the difference in completed fertility in either 1970 or 1980, supporting our hypothesis that access to the Pill did not affect the demand for children. Additionally, although none of the point estimates are individually significant, the magnitude of the difference in 1980 is reduced over the 1970 estimates, suggesting that the impact of access to the Pill on completed fertility continues to decrease over time. Since our omitted category is 1915-1918, and these women

would have been 63 to 65 in 1980, it is possible that we could be picking up differential mortality among these older women if women who had more children were more likely to survive to old age, because they were strong enough to survive multiple childbirths. But as long as this differential mortality is random with respect to access to the Pill, it should not bias our results.⁸⁶

Figure 4.12: Differential Evolution of Children Ever Born (1970 and 1980), by Cohort



Notes: The figure plots changes in the gap in children ever born between women living more than 50 miles from a non-restrictive border and women living closer in the same birth cohort and state, relative to the omitted cohort of 1915-1918. Each series is a separate regression, all including covariates. Confidence intervals are at 95%. Source: 1970 census, metro forms 1 and 2; 1980 census, 5% sample; IPUMS (Ruggles et al, 2015).

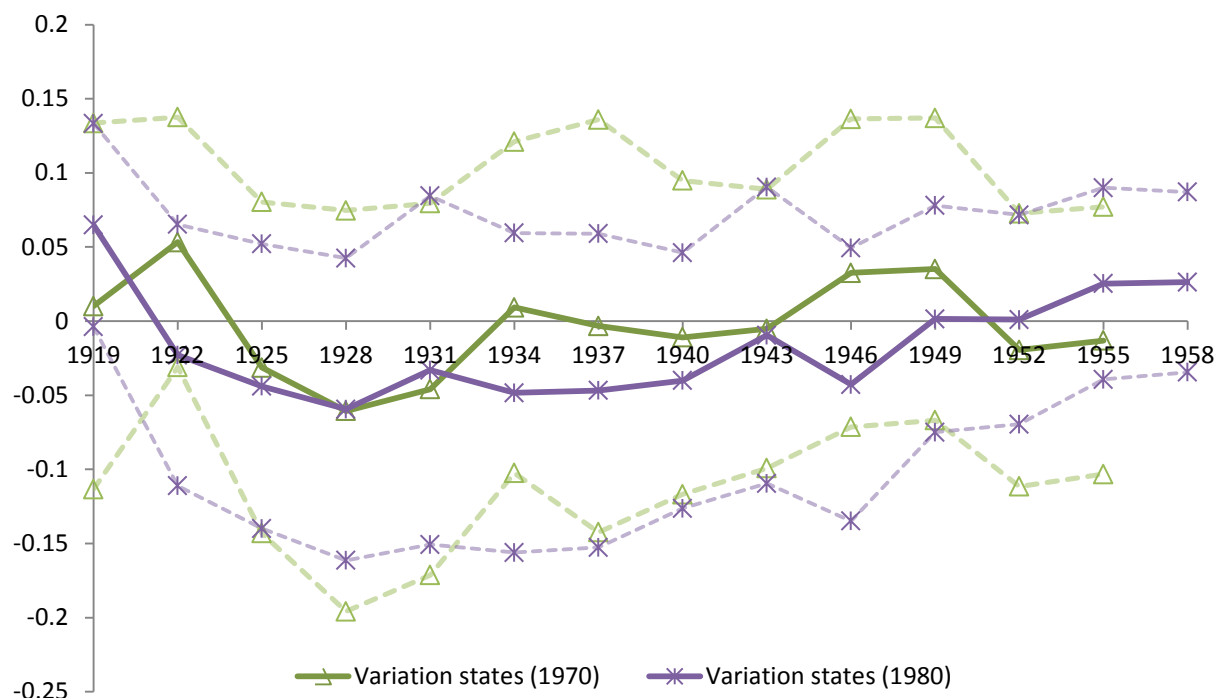
It is initially surprising that there appears to be no long-term impact of more costly access to the Pill. It is important to realize, however, that the theoretical relationship between the Pill and completed fertility is not straightforward. According to the Michael and Willis

⁸⁶ We believe is plausible since these women would have been 39 to 42 and nearing the end of their fertility when the Pill was first introduced, and thus unlikely to have benefited from it.

(1976) framework for understanding the effects of contraception on fertility, access to more reliable contraception should have two separate effects on completed family size. First there is an effect on unintended pregnancies, which should be unambiguously negative since the failure rate of the Pill is lower than that of any other form of contraception, save abstinence. Second, there is an effect on intended pregnancies, which is expected to be positive. If each couple derives more negative utility from a family greater than their ideal size than from a family less than their ideal size, we would expect that in the case of faulty contraception the couple might tend to stop having children before they reach their optimal number of children, due to the risk of overshooting. In this case, access to reliable contraception might actually *increase* the number of children ever born, because it reduces precautionary undershooting. As a result, the effect of the Pill on completed fertility is theoretically ambiguous.

If easier access to the Pill did reduce precautionary undershooting, and couples' ideal family size remained unchanged, we might expect to see decreases in the variance of children ever born, as couples with access are better able to exactly hit their target family size. Figure 4.13 plots point estimates for regression (4.4) with the standard deviation of children ever born as the dependent variable. Contrary to our hypothesis, we find no measurable decrease in the standard deviation of children ever born among women with easier access to the Pill.

Figure 4.13: Differential Evolution of the Standard Deviations in Children Ever Born, by Cohort



Notes: The figure plots β_3 coefficients on d50 interacted with birth cohort dummies from regression (4.4) for women in restrictive states with variation in the 1970 and 1980 censuses. Dependent variable is the standard deviation of children ever born in each county group. Each series is a separate regression, all including covariates. 95% confidence intervals are shown. The displayed year is the midpoint of the 3-yr birth cohort. Source: 1970 census, metro forms 1 and 2; 1980 census, 5% sample; IPUMS (Ruggles et al, 2015).

4.5 Conclusion

Building on Bailey (2010), we use a distance-based treatment measure to capture within-state between-county group variation in the costs of access to the Pill. This new empirical strategy is significantly less likely to be correlated with state-level unobservables, and thus potentially better able to capture the true causal impact of access to the Pill. We show that this distance measure captures most of the state-level variation in Bailey (2010), and that the inclusion of state-by-year fixed effects suggests that the differential cost of access driven by distance to a permissive border did not yield measurable changes in period fertility. We find slight suggestive evidence that lower-cost access to the Pill may have decreased childbearing

during the period of differential access for women born between 1936 and 1941, but that any decrease is primarily driven by delayed childbearing. We find no statistically significant differences in the total number of children born 5 and 15 years after the costs of access to the Pill were equalized, nor any impact on the standard deviation of children ever born. This suggests that any impact of access to the Pill is one of timing and spacing, and that access does not significantly affect completed fertility. These results support the traditional view of economists that the decline in fertility following the baby boom was driven by changes in the demand for children, not by access to the Pill.

4.6 References

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